Knowledge.

With which is incorporated Hardwicke's Science Gossip, and the Illustrated Scientific News.

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Conducted by Wilfred Mark Webb, F.L.S., and E. S. Grew, M.A.

APRIL, 1911.

SOIL BACTERIA AND CROP PRODUCTION.

By H. B. HUTCHINSON, D.Sc.

DURING the last thirty years the methods of estimating soil fertility have undergone considerable change. The crude chemical method of determining total soil constituents has given place to one more nearly approximating that of the plant itself, and the value of the results so obtained has been considerably enhanced by the consideration of the mechanical composition of any given soil.

Within the same period the old conception of the soil as being merely a store-house of plant food has given way and soil is now regarded rather as a medium in which myriads of micro-organisms exist and exert their influence. The dissolution of plant and animal remains and the consequent production of substances capable of subserving plant growth is the work of soil bacteria. Since nitrogen is the most valuable soil constituent, it is not surprising that the organisms responsible for various reduction and oxidation changes such as ammonification, nitrification, nitrogen-fixation, and so on, have received most attention, while those inducing cellulose fermentation and changes in sulphur and iron compounds have been less studied.

FIGURE 1.
Wheat Growing in Partially-sterilized Soils.
Left to Right: Untreated, Toluened, and Heated Soil.

Although the activities of this micro-flora result in the production of plant food, it would be fallacious to assume that this represents in any adequate measure the total changes occurring in the soil. Decomposition of highly-organised nitrogenous bodies may be accomplished, nitrates formed, and nitrogen may be fixed, but a portion of this elaborated material may be lost again by the action of other organisms. Symbiosis and antagonism between soil organisms have been observed in many instances, and recent investigations indicate in a striking manner the far-reaching effect of another factor inimical to bacterial growth.

The extent to which this factor exerts an influence is shown by results obtained from experiments on the partial sterilisation of soils. Numerous workers have shown that if a soil is subjected to the action of heat or of mild antiseptics, an increase in the fertility of that soil ensues. In the former case high temperatures (95° to 100° C.) were adopted, and no doubt led to a certain amount of chemical decomposition of some of the soil constituents. Direct chemical action, however, is insufficient to account for the increase in fertility.

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The use of volatile antiseptics seems to have been first introduced by Oberlin for the destruction of the phylloxera disease of the vine. The favourable results obtained by the use of carbon disulphide led to trials with other crops with equally good results. Obviously, the benefit could not be due to any diminution of plant diseases, but to a stimulation of processes concerned in the nutrition of the plant, or of those occurring in the soil itself. Koch regarded this increased fertility as being due to a stimulation of the plant by small quantities of the antiseptic remaining behind in the soil after treatment, while Hiltner and Störmer attributed it to the destruction of nitrate-decomposing bacteria, and to a subsequent growth of humus decomposers—the various species of Streptothrix. Further it was supposed to be due in part to an increased growth of those bacteria which produce nitrates or assimilate atmospheric nitrogen.

For some time this problem has been studied by Dr. E. J. Russell and the Author, and the probability of the existence of an altogether different factor has been indicated. Our initial experiments were designed to show the actual increase in fertility as measured by plant growth. Samples of soil were carefully sieved and filled into glazed pots, which were then divided into three sets. One set remained untreated, a second set was heated to 100° C. for three hours, while a third was partially sterilised by the addition of four per cent. toluene and afterwards freed from this substance by exposure to the air. Various crops were grown in these soils—buckwheat, rye, mustard and wheat—and the results obtained with rye may be taken as typical. Table A gives various data as to the weights produced, while Figure 1 is from a photograph of some young wheat plants growing in the respective soils.

The use of partially sterilised soils for plant experiments has two distinct effects; there occurs a striking increase in the weight of crop, varying from twenty to over two hundred per cent. and, further, there is almost invariably an increase in the nitrogen content of such crops. Other experiments have shown that the latter is due to an assimilation of nitrogen as ammonia instead of as nitrates as is normally the case; mild antiseptics and moderate heat are sufficient to destroy the nitrifying organism. In order to follow the various chemical and bacterial changes, samples of the respective soils were stored in sterilised bottles and analysed at certain intervals. Chemical analyses revealed the fact that a rapid decomposition of the nitrogenous constituents of the treated soils, with ammonia production, was taking place, while the untreated soil slowly produced nitrates. Bacteriological analyses demonstrated an initial drop in the number of organisms present, with a subsequent rapid increase, until at the end of a few weeks the toluened soils contained five to eight times the number of bacteria as the untreated soils. These changes are shown in Table B.

The connection between bacterial activity and the production of plant food is obvious from the above

Soil.	Weight of Green Crop.	Weight of Dry Matter.		Percentage Composition of Dry Matter.		
		In Grams.	Relative Weights.	N.	P_2O_5	K_2O.
Untreated	103·95	37·14	100	·698	·59	1·05
Heated	162·10	59·30	160	1·147	·64	1·28
Toluened	120·07	44·76	120	·742	·54	1·01

Table A.

Treatment of Soil.	Total Nitrogen as Ammonia and Nitrate. Parts of Nitrogen per Million.			Bacteria per Gram of Soil. In Millions.		
	At beginning.	After 23 Days.	Gain.	At beginning.	After 23 Days.	Gain.
Untreated	13·8	17·7	3·9	6·76	9·81	3·05
Heated (98° C.)	19·5	55·8	36·3	·000007	5·23	5·23
Toluened	17·0	39·8	22·8	2·84	40·62	37·78

Table B.

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results, and the assumption that increased fertility was due to a bacterial rather than a chemical cause, appeared justified. Such being the case, a closer study of the bacterial flora of the various soils was undertaken and an attempt was made to account for the enormously increased numbers. Definite quantities of the soils were introduced into solutions containing peptone, urea, hay-dust, and so on, and the amount of ammonia produced after certain intervals was estimated. In almost every case it was found that the cultures containing toluened soil were more capable of decomposing the substances supplied. This result could be induced *(a)* by a greater virulence of the toluened soil bacteria, or *(b)* the presence of some inhibiting factor in the untreated soil. Experiments where toluened soil was inoculated with bacteria from untreated soil gave, however, a more vigorous decomposition, and a higher bacterial content than a toluened soil alone, thus showing the superiority of normal soil bacteria over those persisting after treatment with the antiseptic. This point having been cleared, it became necessary to search for a limiting factor in normal soils. It was not possible to demonstrate the presence of injurious bacteria nor that of any toxic body. On the contrary, an extract from untreated soil, that is, liable to contain any soluble toxin or toxic organisms, produced a vigorous decomposition, but similar experiments where toluened soil was inoculated with small quantities of untreated soil, showed the limiting factor to be transmissible in soil itself.

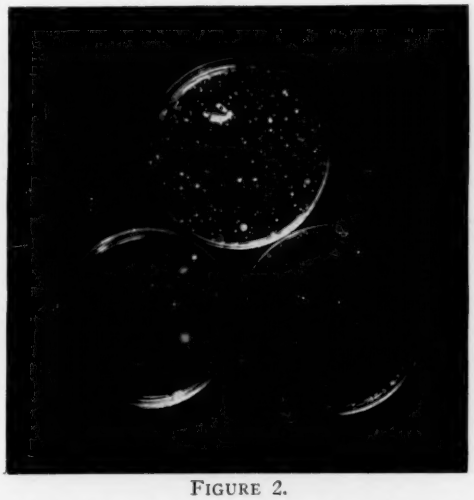

FIGURE 2.
Gelatine Plates showing relative numbers of Bacteria in Untreated, Toluened, and Heated Soils.
Bottom, Left-Hand: Untreated Soil. Top: Heated Soil. Bottom, Right-Hand: Toluened Soil.

When, for instance, toluened soil was mixed with five per cent. of untreated soil there ensued an initial increase in bacterial growth and ammonia production, but these changes were retarded in the course of a few weeks. (See Table C.)

It is apparent from the above, that, while considerable bacterial growth has proceeded in three out of the five soils, it has remained stationary in the untreated soil and toluened soil with five per cent. untreated soil. This condition, however, only begins to be evident some weeks after the beginning of the experiment. In fact, during the period up to the thirty-eighth day, the toluened soil receiving untreated soil possessed the highest bacterial content. In brief, the limiting factor appeared to be of a biological nature, possessing the following properties:—(*a*) destroyed by heat and volatile antiseptics, (*b*) incapable of passing through filter paper, (*c*) contained in small quantities of soil, (*d*) capable of growth, and of reducing the numbers of bacteria. It appeared to exert a limiting effect either by competition for nitrogenous foodstuffs, or by destroying the bacteria in the soil. A search was then made for organisms larger than bacteria, and, by the aid of special media, protozoa were found in the untreated, but not in the treated soils. The influence of soil protozoa upon bacterial change was demonstrated by adding cultures of these organisms to flasks containing extracts of untreated and toluened soils and peptone solution, and estimating the amount of ammonia produced after a given period. In this manner we were able to depress the activity of the toluened soil bacteria

Treatment of Soil.	Bacteria per gram of Soil.			Total Gain in Nitrogen as Ammonia and Nitrates after 57 Days.
	After 38 Days.	After 61 Days.	Gain.	
1.—Untreated Soil	7,500,000	9,500,000	2,000,000	2·5
2.—Toluened Soil	31,800,000	60,100,000	28,300,000	24·3
3.—Toluened Soil + sterilised extract of untreated Soil	31,600,000	67,000,000	35,600,000	23·4
4.—Toluened Soil + non-sterilised extract of untreated Soil	45,200,000	166,600,000	141,400,000	43·7
5.—Toluened Soil + 5% untreated Soil ...	46,900,000	48,000,000	1,100,000	20·3

TABLE C.

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to a point below that of the untreated soil bacteria.

Many different species of soil protozoa have already been isolated, chief among which may be mentioned Pleurotricha, Colpoda, Vorticellids, Amoebae, and Monads. A large number of soils have been examined, and these organisms would appear to be of universal occurrence; their number varies with different soils, and at different depths of the same soil.

A study of their behaviour towards various physical and chemical agents has suggested fresh lines for investigation in connection with soil fertility. Experiments have shown the thermal death-point of protozoa to lie between 43° and 48° C, and it has been found that if a soil be heated to 50° C. instead of 100° C., as formerly, a change occurs similar to that produced by toluene. The relations of protozoa to intense cold or thorough desiccation are still being investigated. It is commonly believed by practical agriculturists that a sharp hard frost conduces to increased crop production and in the light of the above results it would appear to be justified, as extreme cold for a short period is sufficient to kill many protozoa. The Indian practice of cultivating and pulverising the soil during the hot dry season would appear to rest upon a similar foundation.

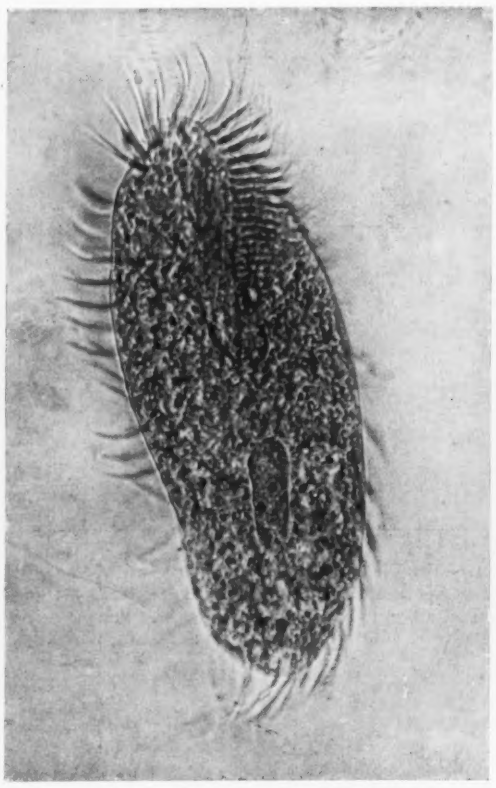

Figure 3.

From a Photo-Micrograph (Pleurotricha, one of the Protozoa occurring in Soil).

In this country where extremes of cold, heat, or dryness scarcely ever prevail for any considerable period, any effectual method of partially sterilising the soil would necessitate the use of cheap antiseptics or the application of temperatures easily obtained in market gardens and glass houses.

Whatever success may be gained in the adoption of the methods of partial sterilisation on a large scale, it becomes evident from the above results, that a fresh factor must be recognised in the estimation of soil fertility. In the meantime much work remains to be done in the study of the relations existing between bacteria and protozoa and the conditions governing their growth in the soil.

METEORIC ASTRONOMY.

By W. F. DENNING, F.R.A.S.

The claims of meteoric astronomy as an attractive field of observation, and as a subject of investigation likely to expand our knowledge, are great—yet it must be confessed that it is not studied as it should be.

A large amount of useful work was accomplished by the British Association Committee on luminous meteors, between 1848 and 1880, and certain individuals have contributed a considerable amount of data to this department, but much more remains to be done. The southern sky is still practically unexplored.

Meteors not only permeate our solar system but probably circulate far outside that and may indeed pervade the whole universe of stars. If the latter are suns with planetary satellites then fragmentary atoms probably abound, some in the form of cometary swarms, others in more tenuous streams. In fact, wherever planetary systems exist we may take it that meteoric particles, forming as it were the dust of such systems, are also present in considerable numbers. And across the wide and incomprehensible abyss separating one sun from another meteors may roam in hyperbolic or parabolic orbits. The far-reaching relations of these objects, their vast numbers, and cometary associations render them of extreme interest and importance. We must learn more of the phenomena which they present in our terrestrial skies, so that we may judge of their character and behaviour in the remoter regions of space.

For the present it seems that the photography of meteors has failed to supply us with an effective and accurate means of recording their flights on ordinary nights of the year. We must continue therefore to follow the old and rough method of registering paths and determining radiants. When we consider, however, that radiation forms an area not a point, and that eye-estimates of the directions of flight may, after sufficient practice, be made with a precision almost equal to photographic trials, the latter has really only a slight advantage.

The chief periodical showers of the year have been watched for many years, and a large amount of evidence concerning their displays has been accumulated. Still the materials existing are far from being of the character or extent required. We want more data as regards the long duration of showers and as to their stationary or shifting positions of radiation. The radiant points of the principal streams should be determined on ten or more nights near the date of maximum in cases where the showers are active so long as that.

RECENT INVESTIGATIONS ON AURORA BOREALIS.

By OUR BERLIN CORRESPONDENT.

ACCORDING to a hypothesis suggested by Professor Birkeland of Christiana University, auroras are due to cathode rays given out from the sun, and which on their way through the cosmic space would converge towards the magnetic poles of the earth, thus producing a bright fluorescence in the surrounding air.

In fact, when arranging below a Crooke's bulb a very strong magnet, the cathode rays are seen to converge towards this, like light rays converging towards the focus of a lens. This phenomenon Professor Birkeland denotes by the name of "suction effect" of the magnetic pole.

When a discharge bulb with a minute "magnetic earth" suspended in its interior is lined with a layer of platinum-barium cyanide, any spot struck by the cathode rays becomes distinctly visible.

By varying this experiment, there are obtained the most manifold fluorescent forms reminding in all details of auroras. In order to test his theory, Birkeland also undertook three voyages of discovery to polar regions, from which he brought home many valuable data on the aurora borealis and the concomitant magnetic disturbances.

His colleague, Professor Carl Störmer, of Christiana, in a memoir recently submitted to the Fourth International Congress of Mathematicians, then established a theory of the phenomenon, showing all its details to be perfectly accounted for on the above hypothesis. He also worked out a new method of aurora investigation by photographic records.

In view of the inadequate luminous intensity and great mobility of auroras, it had so far been considered impossible to fix the phenomenon photographically. Professor Störmer, however, realized that by choosing a proper combination of objectives and photographic plates, sufficient sensitiveness could be insured. By means of a cinematographic objective one inch in diameter and two inches in focal distance and violet-labelled Lumière plates, he then succeeded, on a voyage to Bossekop (Finmarken), in February and March 1910, in obtaining four hundred satisfactory aurora photographs out of a total of eight hundred, with exposures varying between a fraction of a second and twenty seconds, according to the luminous intensity of the phenomenon.

One of the most valuable uses these photographs can be put to, is measuring the altitude of auroras and ascertaining their accurate position in the cosmic space. To this effect the position of the aurora in regard to the surrounding stars should be compared on two photographs taken simultaneously from two stations connected by telephone. A systematical application of this method (a report on which was recently presented to the French Academy of Sciences) will doubtless give the most valuable results.

FIGURE 1.

FIGURE 2.

The Aurora Borealis.

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SCIENCE IN EVERY-DAY LIFE.

By Rev. H. N. HUTCHINSON, B.A., F.G.S., F.Z.S.

In the opinion of the present writer, one of the first things to be done by women of the upper and middle classes, is to abolish the private kitchen. Such a plan would save space in the house; a sitting-room for domestics is highly desirable, and the kitchen could be made into such a room. It would save a vast amount of trouble, anxiety and worry. In every street there should be a large kitchen and confectioner's shop. The lady of the house could, each day, order her meals by telephone, and the food could be sent out in wooden boxes for each meal, just as dinners and suppers are provided by the college kitchens of Oxford or Cambridge. The writer has advocated this plan for thirty years; but the habits of a nation are not easily changed. The waste involved in private kitchens is enormous. Food would be of the best under this plan, and it would afford a much greater range of choice. Companies could be started to supply this great want, and contracts might be made quarterly or yearly. Let the ladies seriously consider this proposal, for it is certainly within their power to devise ways of carrying it out.

The danger of fire is considerable, and yet few householders take any precautions to diminish the risk. There is no need to go to any great expense; for hand fire extinguishers can now be obtained at reasonable prices. Two or three fire-buckets might be placed on a landing. A hose-pipe from the bath-room might be effective at the beginning of a fire. In tall London houses, a good plan is to provide one of the top bedrooms with a long rope, so that the inmates could let themselves down to the ground. A stout hook should be fixed just above the window.

Young people are inclined to be careless about their clothing, and in this matter parents should see to it that their sons and daughters do not risk their lives by wearing thin garments in the spring time, or in the cool evenings of May and June. By such simple precautions much lung disease, rheumatism, and so on, might be avoided. It need hardly be said that certain fashions, *i.e.*, high-heeled boots, are scientifically wrong and absurd. But probably no considerations of this kind will have the slightest influence in modifying a foolish fashion, involving serious danger to the human body. We therefore pass on to the subject of "air." People talk much about ventilation, especially in public rooms and places of entertainment, but at the same time such people often pay little attention to the ventilation of their houses or flats, or it may be lodgings. In all our big towns thousands of people live much of their lives in stuffy little rooms, where they cannot possibly get all the air they need to keep in health and to digest their food. Of late years there has been marked improvement in this matter, owing to the greater use of electricity for lighting purposes. But one still sees many rooms (often of but small size) lighted by gas. Now this is really a grave evil, and the cause of an immense amount of ill-health and suffering. Those who live in such rooms are simply breathing poisoned air, all the time that the gas is burning! What it means those who have learned a little chemistry can readily comprehend. Each gas-burner is pouring into the room a constant stream of carbonic acid and sulphur dioxide, two poisonous gases. Fortunately a little air enters from under the door, drawn by the heat of the fire, otherwise the inmates of the room would be suffocated. Such a use of gas ought to be strictly forbidden. By a simple plan, which the writer has often advocated, this grave evil can be avoided. Any plumber or builder could easily devise a method of drawing away all gaseous products of decomposition. One plan would be to have the lights in the ceiling, but enclosed, as is often seen in railway carriages. Another way would be to provide a fairly large glass or metal funnel to hang over each gas bracket—to the top of this funnel a pipe could be fixed of sufficient diameter to take away all the foul air, either into the chimney or through a hole in the wall. Central chandeliers might be taken away altogether, and their place supplied by the ceiling light, as described above. A third device would be small brackets on the walls, each enclosed in an artistic glass case, with pipes to conduct the bad air to the chimney, or through the walls to the outside. In this simple manner the air of our rooms might be kept just as pure and fresh as in cases where electricity is the lighting agent. No matches need be used, for every burner would be provided with a by-pass, for lighting and for turning down, and, of course, incandescent mantles should be adopted on account of the increased light they afford. Builders of small houses should be compelled by law to provide for some such plan, or, at least, gas-fitters should be forbidden to fit gas-burners in such a way that the products of combustion cannot be led away from the rooms. If householders would adopt this simple plan they would not only get their rooms lighted cheaply, but they would help to make it more largely used, and so, before long, obtain a considerable reduction in the price thereof; also explosions would be almost impossible.

With regard to the heating of houses we are working on wrong lines. The usual method is unsatisfactory in every sense (except artistically) and quite unscientific. So here is a matter in which reform is greatly needed. The use of coal in fire-places not only causes smoke with all its grave evils, but involves a waste that is truly prodigious! We

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must consider not only the waste of useful carbon and so of heat—that is bad enough, especially in its indirect results—but we have to consider the loss of those extremely valuable bye-products that might be obtained if the same fuel were heated in retorts and made to yield up its gas. The wealth of England is partly due to its splendid coal-fields. Coal being part of the national wealth, it should be the aim of all good citizens to prevent the waste of it. But as things are we allow a large part of our wealth to escape up the chimney. This is bad science and worse political economy; it means we are spending capital as well as income; but unless the women will approach the subject in a different spirit there is not much prospect of reform.

To make the matter clear, it will be necessary to explain very briefly what happens in the manufacture of coal-gas. Coal is heated in closed vessels called retorts and the gases driven off led away into gasometers, and thence to the mains that supply our houses. Now the three chief residual products of this process are: coke, an ammoniacal liquor, and coal-tar. The coke is principally used in manufactures; its value depends on the kind of coal from which it is got: some kinds yield a coke of great value commercially, the gas companies deriving a large income from its sale. To some slight extent it is used in domestic grates, especially in kitchen ranges. But the second product, ammoniacal liquor, is still more important. A ton of cannel coal will yield eighteen to twenty pounds of ammonia (in the form of sulphate), ordinary coal yields about sixteen pounds. The chief use of the sulphate of ammonia is as a fertiliser of soils, and for this purpose it realizes good prices. The third bye-product is tar liquor. This substance yields, by distillation, a wide range of products of great and increasing industrial value. In the process some highly volatile products are given off—consisting principally of benzol and afterwards a large amount of light oil called "naphtha" (a mixture of hydro-carbons). At this point the residue in the retort is called "artificial asphalte," and as such has a commercial value. But if the heat is forced and distillation continued a large amount of heavy oil is obtained, and the mass left in the still is hard pitch. The heavy oils are a mixture of naphthalin, phenol (carbolic acid), cresol (cresylic acid), and anthracene and so on. The benzol obtained in the first stage of distillation is the basis of aniline and its various dyes. Naphtha is used as a solvent and in other ways. Carbolic acid is largely used as a disinfectant and also is the basis of many valuable dyes. Anthracene is the basis of a very valuable dye called "artificial alizarin" and most of the above substances have other applications of minor importance. The following figures, kindly supplied by the secretary of the Gas Light and Coke Company, may be quoted here. For the year ending June 30th, 1910, this company purchased coal to the value of £1,052,000. The revenue from coke was £536,000. Sulphate of ammonia and cyanogen products yielded £184,000. From the latter are obtained sodium cyanide, largely used in gold mining, Prussian blue, and so on, used for other chemical processes. The other bye-products *e.g.*, tar, pitch, creosote, benzol and anthracene brought in a revenue of £95,000. It will thus be seen that the total revenue from bye-products amounted to the large sum of £815,000, *i.e.*, over three-quarters of the value of the coal used.

The above very brief account suffices to prove that several important industries depend upon gas-making. Consequently the more gas is used for "domestic" heating, cooking and lighting, as well as providing motive-power by working gas engines, the more men and women will find employment in these industries, thus at the same time earning a living for themselves, and increasing the natural wealth of the country.

In the last ten years or more, enormous improvements have been made in the domestic fire grate: it is now not only more artistic, but much more effective and economical, and thus a further step has been gained in diminishing smoke production in large towns. Many attempts have been made to construct a smokeless domestic grate, but this is almost impossible; for when once the heavy carbonaceous smoke has been produced, it is very difficult to burn away the carbon particles completely, on account of the large volume of nitrogen present with the oxygen in the air passing up the chimney. So that the best method of preventing smoke is to put on the coal in very small quantities. Another good plan is to burn wood or coke with the coal. The use of anthracite coal would result in a smokeless and very hot combustion, but it is difficult to light and also requires a special stove. Consequently the initial cost largely stands in the way of the general use of anthracite for domestic use, and also it is obvious that any great demand for such coal would create so great a rise in prices as to render its use prohibitive, the supply being limited. The use of half-baked coal is not a new idea, but since "Coalite" was put on the market attention has once more been turned to this method. The "Coalite" process has one advantage, viz., that the fuel is of greater uniformity, and the yield of tar is doubled, instead of being decreased. In the opinion of a leading chemist, "Coalite" will be the ideal fuel for home use, but to the present writer it seems that the only satisfactory solution of the problem lies in the abolition of coal-fires, their place to be taken chiefly by gas-fires and gas-stoves. This is, undoubtedly, the proper scientific way of solving the smoke problem, and of preventing the waste of precious coal. Not that it is the only solution, for in larger houses, hotels, public buildings, colleges, and such places, hot water heating is almost a necessity. The furnace in those cases might be constructed to use gas instead of coal. Electric radiators might with advantage be used in small rooms, and for warming odd corners. At present, however, they are expensive. For some years past, as all Londoners thankfully recognise, London fogs have been much fewer and far less dense than was formerly the case, and it is generally recognised that

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this welcome improvement is chiefly due to the more general use of gas-stoves and gas-fires. Having used gas-stoves for many years, the writer has no hesitation in recommending them. If properly fixed up by a competent fitter, there is an entire absence of smell in the room. At the back is a flue-pipe to carry away the gaseous products of combustion. This should be fairly long—say, three feet at least—and carefully fixed on the stove in such a way as to avoid any leaky joints. This is most important. It is best to fill up all the space between the stove and the surrounding fireplace. Ladies appear to have a prejudice against the gas-stove, which is a pity. They must, however, admit that an immense amount of labour is saved by their use, and that dust is entirely avoided. There is this further advantage (especially in bedrooms), that a gas-stove can be regulated to a nicety, and turned off when no longer required.

Professor Vivian B. Lewes, lecturing on December 8th of last year before the Royal Institution, said: "The principal cause of the cloud which hangs over our big towns, cutting off the direct rays of the sun and ruining health, varies with the locality. In the South of England it is the domestic grate, using bituminous fuel, which is responsible for the major portion of this pollution of the atmosphere; whilst further north, in the great manufacturing centres, it is the factory shafts which emit the pall of black smoke that aids in shortening life and killing vegetation, and which begrimes and finally helps to destroy our public buildings."*

One frequently hears London ladies complaining of the dust and dirt that blackens windows, ceilings, pictures, hangings, clothes, and so on, But it is in their own hands that the remedy lies. Let them abolish coal-fires!

SOLAR DISTURBANCES DURING FEBRUARY, 1911.

By FRANK C. DENNETT.

ALTHOUGH meteorological conditions have been very unfavourable during February, it has been possible to obtain a fairly regular record. Somewhat greater activity has been noted. The surface appeared to be quite free from disturbance upon February 2nd, 3rd and 25th, and none was seen on the 8th and 23rd. Only faculae were visible on the 1st, 22nd and 24th. On February the 1st, at noon, the longitude of the Central Meridian was 246° 18′.

No. 2.—First seen on February the 10th as a fair-sized spot a few degrees from the eastern limb, solitary, with faculae around, but mostly to the east. Upon the 11th and 12th it contained three or four umbrae; the diameter was 14,000 miles. From the 13th until the 15th the spot seemed shrinking, and its umbra crossed by a bridge, with three tiny pores close north to east on the 14—15th. The dwindling spotlet was last seen as a pore on the 20th.

No 2*a*.—A pore amid the faculae about 52,000 miles to the rear of No. 2 upon the 11th. Three or four pores upon the 12th, and next day a curve of pores like part of an elliptical ring, and containing one of a larger size. On the 14th, this group presented a somewhat peculiar appearance—a double umbra partly surrounded by penumbra, and further east, a group of umbrae almost like a capital F, and two or three pores. On the 15th the rear spot was 9,000 miles in diameter, with a bridged umbra and a curve of pores from east to south, whilst ahead four pores outlined the form of a lozenge, and three others remained to the north-east. On the 17th the spot had shrunken to a pore, whilst larger spotlets had developed ahead; making the total length about 66,000 miles. Only one or two pores continued until the 19th and 20th, when last seen. The group was close to the faculic area C. in the chart for January.

No. 3.—On the 11th a group of pores 40,000 miles in length. A double pore acted as leader, having three tiny pores forming a triangle in the rear. On the 12th two pores only, in a faculic setting, which formed pretty curves. Not seen again. It was closely south-east of the place of No 1.

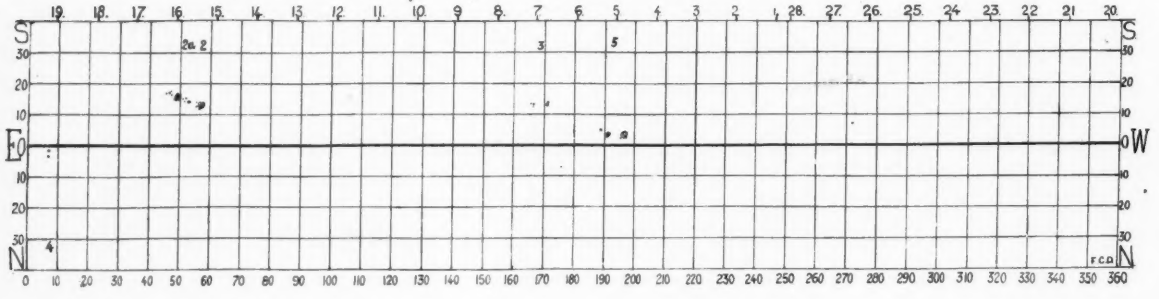
No. 4.—At 9.30 a.m. on the 20th two pores were easily seen and measured, both on longitude 7°, and some 11,000 miles apart. By 2 p.m. both had disappeared.

No. 5.—A single spot amid faculae, first seen near the eastern limb on the 26th. On the 28th there were two spots, and a few pores. The western spot contained four umbrae on March 1st, and during the day pores began showing farther east; on the 2nd the length of the outbreak was 57,000 miles. Only a few pores remained on the 5th, when last seen, but faculic disturbances marked the area as it drew near the limb on 8th and 9th.

On the 24th, on the south-western limb, in latitude 36°, there was a splendid prominence form some 110,000 miles in height.

The chart is constructed from the observations of Messrs. J. McHarg, A. A. Buss, E. E. Peacock, and the writer.

DAY OF FEBRUARY.



* See *Nature*, vol. 85, page 290, December 29th, 1910.

NOTES UPON THE FUNDAMENTAL SYSTEM OF STARS.

By F. A. BELLAMY, Hon. M.A., F.R.A.S.

Owing to the unfortunate delay, mentioned in the last number of "Knowledge," Professor Boss' arranged second visit to San Luiz, with the other members of the observing staff, Mr. A. J. Roy, Mr. Sanford, Mr. Zimmer, Mr. Fair, Mr. Gibble and Mr. Delavan, and with the instruments, was postponed to January, 1909, by which time it was expected that the observatory buildings would be ready for the instruments. Among the instruments were the Olcott meridian-circle, clock, chronographs, photometer, and other necessary apparatus. Immediately on arrival at San Luiz the various instruments were erected, and preliminary work was started. When the necessary adjustments were effected the actual programme of work, carefully arranged at Albany, was begun by the observers, and will be continued until completion; the whole work of observation was then expected to take about three years from the time of commencement (April, 1909). Professor R. H. Tucker will be left in entire charge of the observatory and work. This astronomer, who is probably the most skilled observer in meridional astronomy, is the assistant in charge of the meridian-circle at the Lick Observatory, and has been granted leave of absence from there for the express purpose of assisting in this important investigation. Professor Tucker was an assistant at the Albany Observatory thirty years ago; he was also the chief assistant for many years at the national observatory at Cordoba, Argentina, later on joining the staff at the Lick Observatory, where he has been for ten years engaged in important meridian work. Mr. Roy and Mr. Varnum are the two senior assistants at Albany, and are experienced observers with the particular instrument which is being used at San Luiz; moreover, it was they who largely contributed to the Albany observations of 1907–8; so we may hope for excellent and homogeneous results.

Owing to the extremely dry climate of San Luiz, the vegetation is scanty, except in places where resort is had to irrigation. The plot upon which the observatory stands is under irrigation and is covered with a luxuriant growth of *alfalfa**; the effect of this is of great importance, as it protects the soil and reduces the variations of radiation to a minimum, a most essential point to be considered in astronomical observations at low altitudes. The soil, under the rich surface layer of vegetable mould, is a sandy loam from three to five feet in thickness; under this is a layer of gravel of similar thickness, and below that a dry and hardened clay; this is considered to offer an ideal foundation for the piers of a meridian-circle. Within a week the true meridian was settled, plans were made for foundations, and the site was soon covered with bricks and mortar. The transit room, 22-ft. × 23-ft., is of brick, with a wooden roof; the office rooms, of one story, are of brick with a galvanized iron roof; the exterior dimensions are about 80-ft. by 60-ft., the central portion being arranged on the usual Spanish method with an interior court, or patio. The first stone was laid on October 5th, with a little ceremony, in the presence of officials and friends.

Having given a brief and general account of the causes which made this great piece of astronomical work necessary, and how it became possible to accomplish it, we will complete this article by giving a translation, containing more details, of a paper in Spanish read by Professor Tucker, before the American International Scientific Congress, held at Buenos Aires, Argentina, printed at the expense of the Department of Meridional Astrometry of the Carnegie Institution of Washington, U.S.A. American astronomers are greatly indebted to the munificent generosity of Mr. Carnegie for providing the money, without which this, Pasadena, and other astronomical researches could not be made, or, at least, could not be carried out so speedily or effectively.

The Fundamental System of Stars.†

There are two great and permanent problems in astronomy. One concerns the positions of the bodies visible in the Universe, and the other relates to the form, history, and constitution of those bodies. Of the second problem, the chemical constitution and the history of the evolution of the stars, the sun, planets, nebulae, and comets, are keeping the great telescopes, the photographic plates, and the spectroscopes, and other instruments well employed for their investigation; these instruments are of modern form.

For the first problem the Meridian Circle, though much older, has until now been the most perfect and most used instrument, though of less power, for the determination of the positions of these bodies. For the regular observations of transits of the stars, an instrument of this form has been erected in every case in the great observatories of the world.

* This is a species of lucerne.

† *El Sistema Fundamental de las Estrellas*, para R. H. Tucker, Observatorio de San Luiz, 1910, June 30th; translated by F. A. Bellamy.

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The determination of the positions of the stars is the basis of all that we know of the movements of the sun, moon, planets, and, in fact, of the earth.

To fix the position of the system of the sun and our family of planets among the stars it is necessary to know, with every exactitude, the positions of the stars which serve as landmarks or guide posts in our journey. Not only is it necessary to know the positions now, in the year in which we live, but to compare them with the observations of former years from the earliest times of exact astronomical research. All the stars move: there is no exception according to the laws of our science; the difficulty is to distinguish between all these movements and the proper motion of the solar system. It is necessary to study the movements in all parts of the sky, and observe many stars in every part, in order to resolve the problem of the movement of the solar system. There is not a single place on the earth where all stars may not be observed with the necessary accuracy. At one observatory on the equator of our earth one could observe nearly all the stars; but here difficulties would arise owing to the small altitude of the poles, and one could not make exact observations under such conditions.

For a complete system of stars it is necessary to have at least two places for observation, one in the Northern hemisphere and one in the South. There will be many stars which it will be possible to observe in both places, and those observations will serve to bind or connect the results, and for comparing the conditions and results of a system which shall include all the sky. Already many thousands of observations of stars exist in the southern as well as in the northern sky; those observations were made in the early days when the first observatories were erected in the south. But errors of various kinds appear in all the observations. Some are explained by errors of the instrument, some by errors in the positions of the fundamental stars, others by the methods used in the calculations, others by the methods of observation. In general, these are a class of systematic errors peculiar to an instrument, to a star catalogue, or to an epoch; it is necessary to investigate all their sources.

The observations of the northern sky include similar kinds of systematic error as well; but the observations are much older, and there has been much more time for the study of the errors of method, and for comparing the results with the fundamental methods in use.

Each time that one employs these methods, it is possible to adjust or correct the observations to the same epoch, and likewise we are able to improve the results of the old observations. There is nothing new in that process; it has been used at various times for connecting the results of all observatories in the world, both in the north and south hemispheres. But, in general, the zone of stars which can be observed in both hemispheres has served to adjust or connect the system of the north with the system of the south. It is difficult to extend the range so as to embrace the positions from this zone, without deviation, from one pole to the other. Other difficulties exist and arise when various instruments are used, and when different methods of observation and calculation are employed in obtaining the data used in such a comparison.

Now we have a more complete plan for adjusting or connecting the observations of both hemispheres, by a fundamental method, and we hope that the results of this plan will serve to improve the positions of the stars in all the sky. It will be the foundation from which to compute new positions for our epoch, and the base for studying the old positions.

THE PLAN.

The plan includes:—

(*a*) The observations, *with one instrument*, of all the principal stars in the sky;

(*b*) the use of a fundamental method to fix afresh the positions of those which serve for the basis of the calculations;

(*c*) the investigation of the results of all observations by a uniform method;

(*d*) the comparison with the results of old observations by the use of these new positions;

(*e*) and the binding together into one system all observations from pole to pole.

ONE INSTRUMENT TO BE USED.

This is the plan which forms a part of the work of the San Luiz Observatory in Argentina. It is the first occasion on which one has used a single instrument for observations of this class of work in both hemispheres. The observations for this extensive plan were commenced in the city of Albany, U.S.A., some years before taking the instrument to San Luiz, and, after the conclusion of the observation of the more southern sky, the same instrument will be mounted, at another time, in its old place (at Albany) in order to complete the rest of the observations. The observations of the south will be made, therefore, in the middle of those in the north, so far as relates to the mean epochs. With the results of these observations made upon this plan, it will be possible to correct some of the systematic errors of the old catalogues; as also to form a new basis for a complete system of positions of the principal stars, and a connecting link for other stars which one may include in the same system.

With this object, it is necessary to form a plan of work which will be general and comprehensive, avoiding, as far as possible, the effects of systematic errors in the observations and in the calculations.

It is also necessary to study all the errors of the instrument and apply the proper corrections. Of these the most important are, the division errors or graduation of the circles, errors or inequalities in the form of the axes or pivots of the instrument, and the flexure of the telescope. There are also the corrections of the position of the instrument as in other classes of meridian observations. Besides these,

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it is necessary to investigate the personal errors; one that depends upon the magnitude of the various stars, and the other which depends upon the direction of movement of the stars in the field of view in the telescope. These errors are to be studied for every one of the observers of the commission.

FUNDAMENTAL METHOD.

Of great importance is the use of the fundamental method, in the determination of the meridian line and for the correction of the astronomical clock. In the usual method the principal stars have served for these objects. One used one of the equatorial stars for determining the correction of the clock, and two of the circumpolar stars served for fixing the azimuth of the instrument or direction of the meridian. But in this simple use of the positions of the stars the errors of the positions adopted come into the calculations.

In the fundamental method, however, one does not adopt the positions of the stars, though they are better determined. In all there are some systematic errors; the errors of the observations, and the proper motions of the stars, during the years since they were revised, come in.

Each time that there is a new revision it is probable that the data is more exact. The object of the revision is to adjust all parts of the area of positions in such a way that no differences have entered into the results of the observations when they are made in the various regions of the sky, though it is necessary to use the positions of the stars in opposite parts. In the measurement of small arcs there are no great difficulties, the errors are unimportant; but in the fundamental method it is necessary to measure great arcs, even a complete circumference. In general the errors will be greater, and only by much attention to the observations and calculations can we avoid the increase of these indeterminable differences.

This is the first time that the fundamental method has been employed, to its fullest extent, in a South American observatory. In the fundamental method the direction of the meridian will be fixed by the transits of the same stars made above and below the pole. In this case the error of position adopted will have no increasing influence in the computations.

INSTRUMENTAL CORRECTIONS.

The latitude will be determined by the same circumpolar stars. Other stars which come above and below the pole will serve for a fundamental determination of refraction. For this purpose the stars more distant from the pole, which transit at a small altitude, are selected. It is necessary to study the refraction at each place, especially when the observatory is at a considerable altitude above the level of the sea. For this study we have the combination of the observations at Albany with those at San Luiz. At one place it will be possible to observe the stars which pass in the zenith of the other, and verify, or ascertain, the effects of atmospheric refraction. We use the same instrument with the same circle-division errors, and the tube will be subjected to the same effects of flexure when we measure it in the two places. When we possess the results of the measures extending from the north to the south pole in a complete system, we shall be certain that they will be independent of the errors of the positions adopted for the stars observed. The errors of observations in the zenith will be minute; and it will be possible to correct the observations of the circumpolar stars, by means of the new refraction, in such a manner that those will be more accurate. Also, we shall have a system of fundamental stars in all parts of the sky for adjusting the various parts. These fundamental stars will be observed in conjunction with the zenith and circumpolar stars.

BASIS OF RIGHT ASCENSIONS.

The elimination of the systematic errors in the right ascensions is another problem. A fundamental system should be based upon the position of the sun, finally; but there is no necessity to observe the transits of the sun in connection with all the observations. It is possible to fix the positions of the stars with that of the sun by means of the observations which have been made, and the observations which may be made, without adopting the position of any star whose position is well determined. Observations of the declination of the sun, combined with the observations of the stars, in any epoch, will serve to determine the difference of right ascension of these.

We adopt a system of right ascensions as the basis and with this proceed to the determination of the systematic corrections in the various parts. These corrections have a periodic character; so that, when they occur opposite star groups, with a difference of twelve hours in right ascension, the sum of the corrections will be almost equal, with contrary or opposite effect. From this periodic error the results, when the correction of the clock is determined by the observations of the stars in two groups, at an interval of twelve hours, can be freed of the periodic effect, if not completely, at least in a great measure.

For the change or rate in the correction of the clock, one uses the stars in the same group each day. Between the stars of the same group there is little variation, the positions will be very exact and greatly improved, but all those of a group can have the same systematic error.

For verifying the true meridian-line one can observe by day and by night an electric light, well fixed in the meridian, as an artificial star at a distance of one hundred meters to the north. The position of this light has been determined by observations of circumpolar stars, and one will pursue the determinations of this until the end of the work.

For ascertaining the amount of flexure of the tube of the instrument, one will make observations of the stars by the two methods, by direct and reflected

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view, using a trough of mercury for the reflected image. The use of this method is of great importance in the discussion of fundamental observations. For this class of observations calm nights are requisite, or those with extremely little wind.

Old and New Work.

These remarks aim at explaining the reason of the plan of the fundamental work, the study of all the instrumental and personal errors, the determination of the position of the instrument without using an adopted position of any star, and the determination of the correction and the change or rate of the clock's error by groups of stars in opposite parts of the sky.

This method entails more work, in order to measure from one star to another, than the method mostly in use. It is necessary to continue the observations at least for thirty-six hours, in order to include three groups of stars. The calculations are very heavy.

The corrections of the position of the instrument in a long period may alter sensibly, and it is necessary to study well the progress of all errors. The arcs between the stars are large and the errors of the observations are relatively increased. Finally, it is necessary to observe in the day time when the brighter stars only are within the limits of vision.

For these reasons, and because one can observe the small or fainter stars only at night, the method mostly used is to fix, by differential means, the positions of these fainter objects with reference to the principal stars in their vicinity.

In this manner we have had the principal catalogues, which have been made by degrees, from one part of the sky to the other, just as one has fixed the points on the earth by the difference of longitude between each point and another point about the first or primary. All the longitudes have a single base at last; though all the differences have not been directly measured from the base, it is adopted as the origin of the system of longitudes.

The errors of the differential method can be small in exact observations, yet they have some systematic corrections which increase from one epoch to another.

Now and then it is necessary to revise the scheme, compare the fundamental data, and rectify all the area, with results of great precision and with more complete calculations. In this way the results of the old observations are improved, and these then enter into the modern calculations with less systematic error. Astronomers whom we succeed used those results in the calculations of the movements of the stars and in the movement of the solar system.

Fundamental Method Extended.

The stars which we observe by the fundamental method are now reckoned among those that are better known or determined in the sky. In the first class are included the stars used in this method for clock corrections. The positions of those in the first class, when once well revised are used for fixing the positions of the second class, which is of greater extent, containing the principal stars of all parts of the sky. A third class, in our plan, includes a great number of stars, until now of less value in fundamental work, all well fixed or connected by simultaneous observations. In these classes we shall have altogether 1600 fundamental stars.

By means of the positions of the fundamental stars, one calculates the positions of all the other stars which one observes on this plan. We include the greater part of the stars which have been observed before the epoch 1875·0. The positions of these stars serve to compute many proper motions, and for fixing the direction and quantity of the motion of the solar system. We estimate that we shall observe on our plan 15,000 stars in all.

They are of all degrees of brightness from the first magnitude, besides those more difficult to observe with a telescope of this power. Observations of all are to be found in the catalogues of the past forty years and in the older ones. So, as the old catalogues serve as a basis for the present work, so also will the observations on our plan similarly serve for future calculations.

Climate and Work.

The development of this project has given good results in the observations obtained since the commencement of our scheme of work. In the first complete year we have made more than sixty-two thousand observations. This number has never been reached, for this class of observations, by any other observatory. As the greater part of the work has already been concluded, the task will be easier now.

For the most part the climate of San Luiz has been good. In the first year we had three hundred nights during which one could observe for some hours at least. More than two hundred nights were clear during all hours, Under the usual condition of the sky we could generally reckon upon seventy per cent. of the time for suitable astronomical observations.

A series or cycle of fundamental observations is one-hundred-and-twenty hours, as a general rule, but the observations are continued without intermission. One makes six observations successively of a group of stars and six observations of another group at a distance of twelve hours from the first. These groups are usually made in the evening and early dawn. The same observer continues during some hours at night in order to include the stars which passed in those hours. Other observers follow during the last part of the night for fixing other stars with the principal stars. They are occupied from twelve to ten and six hours for the observations during the twenty-four hours.

The commission has consisted of ten persons for four months only; during the greater part of the work we had six or eight. We are occupied with the computations as well, but these will require many years before being accomplished, and the results of the preliminary calculations are sent to North America, where they will be concluded at the Albany Observatory.

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THE ERECTION OF THE OBSERVATORY AT SAN LUIZ.

The construction and the installation of the observatory has been rapid and without any misfortunes. In September of 1908 we commenced the construction of the building of the observatory in the grounds of the State school. The National Government gave facilities for acquiring the area necessary for the observatory and for a house. Five months later all was established ready for the installation of the instruments, and for lodging the members of the commission.

After the preliminary examination and the determination of some instrumental corrections, we commenced the observations for the scheme in the month of April, 1909.

It is the first occasion upon which an instrument of this class has been erected upon artificial stone, made with Portland cement, grit, sand, or fine gravel, and strengthened with iron.

The two large piers which support the axes of the instrument are joined into one base extended two or three meters, and this base, of two meters in depth, is fixed with the foundation, which is extended beyond the base of the piers; therefore, all is as one block by this modern method of construction. The height of the pillars, with the base, extends to five meters above the foundation, which is of sand and other materials.

There are also two piers of the same construction for auxiliary instruments in the large room, and another pillar at a distance of one hundred meters, for indicating the meridian mark.

The two astronomical clocks are installed within a small room constructed for the work rooms; each clock is arranged on an artificial pillar of the same concrete material.

This is the history of the Observatory of San Luiz. No one here speaks of a work being in progress; it is an act almost completed.

Up to the present we have verified more than seventy-five thousand observations; according to this, five thousand per month since the beginning of our project.

As all has gone off well until this day, we may hope that the work will be completed within a year from the date of 1910, June 30th.

QUERIES AND ANSWERS.

Readers are invited to send in Questions and to answer the Queries which are printed on this page.

QUESTIONS.

32. THE GENUS LINARIA.—Is it possible to obtain a complete list of the species and varieties of the genus Linaria; if so where, and at what price?

G. R. W.

33. "HALLEY'S COMET."—In the March issue of "KNOWLEDGE" it is stated that Halley's Comet is still visible; and of about the 14th magnitude.

Can any of your readers inform me whether the distance of the comet from the Sun can be found by calculation at any particular time, and also whether its speed can be determined?

As an example, how far will it be from the Sun, and how many miles will it move a day, two years after passing perihelion, that is, on April 19th, 1912?

INTERESTED.

34. ORIENTATION OF THE GREAT PYRAMID.—In the articles on "The Great Pyramid" contributed by Proctor to "KNOWLEDGE" Vol. I., he lays great stress on its careful orientation, and devotes considerable space to the method of obtaining a true north-and-south line by means of the descending passage pointing to α Draconis and the ascending passage equally inclined towards the south; but he makes no mention of the means of obtaining a true east-and-west line which must also have been necessary for the construction, perhaps because there is nothing to show what method was actually used. Could a true east-and-west line be laid out by observing a suitable star near the eastern horizon shortly after sunset, and the same star near the western horizon shortly before sunrise? If so, between what latitudes is this method available, and what considerations must be taken into account in selecting a suitable star? What other methods, astronomical or otherwise, are likely to have been available to the Pyramid builders?

W. L.

REPLIES.

24. DREAMS.—Much observation leads me to believe that a large majority, at the very least, of dreams are instantaneous, occupying only an infinitesimal part of the moment of returning consciousness from sleep, and that a noise which wakes a sleeper is itself the cause of the dream which fits it. That, in fact, the dream is an instantaneous "snap-shot" of a commingling of subconscious impressions with those due to external objectives. We must, most of us, be aware how frequently a mere dozy shutting down of the eyelids is productive of a very definite panorama of the mind.

While in dreamland I may say that I doubt strongly the occurrence of the repetition of the same dream in successive slumbers, except as an exceedingly rare event. I believe that the dream itself furnishes the thought of the supposed first dream. Has anyone ever made a note of a dream, and then referred to it when the repetition is believed to take place?

L. J.

30. FINDING THE TIME BY THE HEAVENLY BODIES.—For (1) The determination of the time would depend on the solution of a spherical triangle connecting the hour angle, *i.e.*, the time, with δ, α and λ, the declination and altitude of the sun, and the latitude of the place, α being the angle which has for its tangent the ratio of the lengths of the stick and its shadow. The result might appear rather complex to anyone unfamiliar with spherical trigonometry, and some calculation with trigonometrical tables would be required to obtain numerical results.

The solutions of (2) and (3) are very simple, (2) requiring no calculation, and (3) only the determination of a single angle from the known value of its tangent. Generally if λ is the latitude of any place, and α and δ the altitude and declination of the Sun at noon on any date, then $90^\circ - \lambda = \alpha - \delta$ is always true.

For (2) $\lambda = 52^\circ$; $\alpha = 45$; therefore $\delta = 7^\circ$. The declination of the Sun will be very nearly 7° at noon in London on April 7th and September 5th.

For (3) α will be the angle which has for its tangent the ratio of the length of the stick to the length of its shadow, and the latitude of the place will be $90^\circ - \alpha + \delta$.

The declination of the Sun can, of course, be obtained for any date from the Nautical Almanac; the sign before δ must be reversed if the Sun has South declination.

J. H. G.

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FINGER PRINTS: A CHAPTER IN THE HISTORY OF THEIR USE FOR PERSONAL IDENTIFICATION.

By HENRY FAULDS.

THE famous Tichborne case gave a great impulse to the study of identification as a question in jurisprudence. I was leaving this country for Japan in 1873 and the vast crowd around the old Court at Westminster impressed me greatly with the importance of the subject. Craniology seemed to many to have had its day, and the complexities of constantly varying methods had induced almost complete scepticism. If a race could not be distinguished on anatomical grounds, how could we ever hope to identify a single member of the human family on the basis of anatomy with confidence and precision? It had been decreed, *nemine contradicente*, except Virchow — an important exception — that all the soft tissues, hair, skin, and the like, were now useless for such a purpose. I had studied photographs most carefully, but found them to be traitorous, the same people being made to look quite different in a changed light, by another mode of developing, with the varying psychological moods of the sitter. They were useful but not precise. After certain illnesses, too, the living face was found to change, as in typhoid fever, and more temporarily in ague. The tragic effects of small-pox are well known to novel readers. Our police in England used not long ago to keep an indexed record of tattoo-marked persons who had once been convicted. I doubt if they ever had a case like that of a Japanese once employed by me in their collection. (See plate.) This man's case was unique, I think, his whole skin surface being one finely-wrought pattern, not only intricate but really beautiful. Now such a case might be copied, at great trouble and expense, to win a fine estate. But no one can produce to order the simplest finger-print pattern in living tissue. It may be destroyed, whereas, on the other hand a complex tattoo pattern can be created but can hardly be destroyed. It would be quite impossible by any known means to destroy one like that now figured. Sir Edward Henry says of finger-patterns that they are out of all proportion more numerous than such measurable features as tattoo marks, but I think he cannot have contemplated such cases as that just described.

FIGURE 1.
Enlarged Finger Print showing Sweat Pores.

Along the great populous beach of the Bay of Yedo, where the hospital was which I had charge of, were many shell-heaps or kitchen middens. Some of them were ancient, while others had an almost unbroken history coming down to our own day. Amid the oldest heaps I often found fragments of sun-baked pottery, on which finger-marks had been impressed when the clay was soft. These seemed to have been made by children, perhaps young girls, whose ancient fingers had dinted the edges of the soft ware as pie-crusts are still moulded by the thumb of baker or pastry-cook. Similar articles were then (1878) made and sold as toys, and I purchased many of them in the bazaars of Tokyo. Ancient ware, baked in the sun but never fired, and marked with finger furrows is in high repute for the ceremonial tea-drinking of Japan, but it is quite incorrect to say, as has been said and written, that no other is ever used in those depressing festivities. Sometimes the furrows or ridges of those ancient finger-marks came out sharp and clear, but much oftener they were blurred or smudged by movement

during the act of impressing. In the modern toys, however, the imprints were better impressed and were obviously intended for ornament. Had early men some faint knowledge of the ornamental qualities of finger-patterns? One fancies one can detect some glimmerings of that conception in crude savage designs, but I must not dilate on that tempting theme here. Endowed, or afflicted, with myopic eyes I was led very early to notice how, in the modern ware, one peculiar pattern of lineations would reappear with great persistency, as if the same artist had again left her sign-mark on her work.

FIGURE 2.

Rugae or ridges on the under surface of the prehensile tail of *Ateles ater* (Spider Monkey).

I examined directly many thousands of living fingers, then passed on to consider impresses on putty, bees-wax, sealing-wax, clay, and other substances, taken from my own fingers, those of students under my care, and medical men, native and foreign, and out-patients who might visit the hospital. These were at first very roughly classified and analysed. I am quite sure that at this point the conception of a wide and general method of identification flashed upon me with suddenness. Almost immediately followed a most depressing sense of moral responsibility and danger. What if someone were wrongly identified and made even to suffer innocently through a defective method? It seemed to me that a great deal had to be done before publicly proposing the adoption of such a scheme. Till then we had used wax and other plastic substances (and on the whole paraffin was found to be best), but now I remembered lessons on botany I received in Anderson's College, Glasgow, as a lad attending business. The course—an evening course—cost two shillings and sixpence for the session. We used to print the leaves got in Saturday afternoon excursions with an oily mixture of burnt cork. Using good printing ink in Japan, then, we got large numbers of clear and excellent finger impressions. Their variety was wonderful, and we could study details with much greater ease and delicacy than in *relievo* impressions. From that stage onward I made steady observations, seeking specially to determine whether the patterns characteristic of one individual ever varied from time to time, either in general arrangement or in linear detail. At this time I had noticed that the pigment in human freckles and in the skin affection called *leucoderma* (supposed by some to be the "white leprosy" of the Hebrews) migrated, as my teacher, Lord Lister, had shown to be the fact with the pigment on a frog's foot. The mode I took to test whether the ridges ever shifted their situation or changed their form was by shaving away their elevations or rubbing them down with various powders to smoothness, having first taken careful imprints of the patterns. After the skin grew up again fresh imprints were taken and compared with the old ones. These were scrutinised very carefully for changes, but in many hundreds of cases, tested thus three or four times, not one solitary example of a variation in pattern was detected.

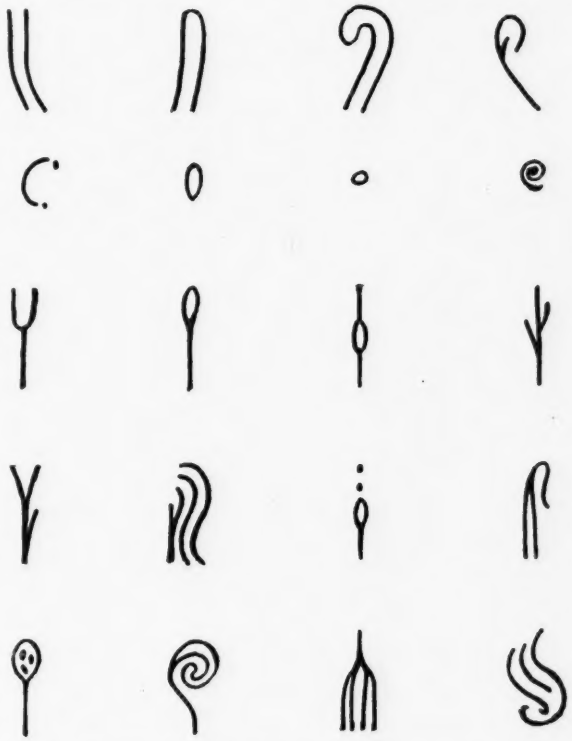

FIGURE 3. Skin Lineations (diagrammatic).

The patterns always came up with perfect fidelity to the old standard. Arrangements were made for a still more extensive test extending also over a greater period, but exhausting illness from climate and overwork caused my return to England, and broke for a time the thread of my investigations. I returned to Japan after a rest, but had again to come back to England in 1886. The firm conviction, however, was established in my mind, which nothing has occurred to change, that skin furrows for the purposes of identification are invariable throughout adult life. Observations of select cases from that period—thirty-two years ago—till now have been made from time to time only to confirm my early results. Figure 1 is one of my earliest prints. In fourteen years it had not changed in the living person. From time to time I have watched cases of fever, and have drawn medical attention to the subject, thinking the great activity of the skin shown by peeling or desquamation might be accompanied with some changes of pattern, but no case has yet been observed by myself or recorded by others so far as known to me. The subject of classification now presented itself. Those who talk glibly about comparing a single "thumb print"—the favourite digit—with, say, four millions of single finger prints, do not seem, as a rule,

FIGURE 4.

Smudge from a Finger.

to perceive the difficulty. The supposed numbers of the Russian army were often discussed in Japanese journalism in the seventies, and it seemed that a good system should be able to face such numbers. Five years of early life had been spent in learning a trade—that of Paisley shawl manufacture—which almost vanished before the end of my time. It seemed to have been an utterly wasted time, leading to nothing and helping no one; but it had drummed into my dull head how to deal with patterns. What I intend to convey by a pattern in finger prints may best be understood by looking at a few enlarged diagrams of the central or most characteristic portion of the *rugae*, or skin-ridges, with their complementary furrows or *sulci*, which are found running over the sole of the foot (*planta*), the palm of the hand (*vola*), and front or palmar surface of the fingers. Indeed, they are even found in the prehensile tail of the spider-monkey, as in the diagram. (See figure 2—spider-monkey's tail.) Near the middle of the last joint of each finger there are usually lineations in the skin of much complexity, which form the basis of identification by finger prints. (See figure 3).

Without going into details, which would require a wealth of illustrative figures and would probably interest but a few, my system proceeds on the conception that an elementary pattern is like a character in a foreign fount of type. So the classification is that of a syllabic dictionary, each syllable standing for a single finger-pattern as a Chinese character is printed in many dictionaries, and as Japanese is now printed. Each vowel may be a syllable in this sense, but no consonant stands alone, and the vowels associated with consonants always preserve their original pattern significance. The consonants go in related pairs, as t, d; p, b; f, v; s, *z*; l, r; m, n; k, g. The elements that compose patterns of any complexity are similarly related in pairs, and thus the association of sound and sense soon becomes complete in the mind of the dactylographer. But the syllable, after all, only denotes a class which may contain several—usually not a great many—individuals, all differing in minute details. With my system the whole strain of the original translation into the finger-print vocabulary, which is never great, lies upon the shoulders of one or perhaps two experts, but all the rest of the work can be done by any school-boy who can turn up a word in a dictionary. To give an example offered to the War Office Committee by me when being examined as a witness; the expert, reading off a new set of say five finger-prints in one hand which has come in, calls for all old records filed past containing *Abracadabra* (a fanciful word of five syllables). That word in syllabic form might read

A–bra–ca–da–bra, or

Ab–rac–ad–ab–ra, or

Ab–ra–cad–a–bra, and so on.

Under any of these forms there might be several people indicated. But it would be found that only one, if any, would correspond exactly with the person to be identified. At all events, that is the belief, not easily to be shaken, of some hundreds of experts working for now about a decade, in different countries. This means quite an extraordinary security, beyond anything hitherto conceived, in regard to personal identification, but its efficiency does not depend on any one method of classification.

Having got a trustworthy method of arranging the records, I now had made copperplate forms to receive impressions of the fingers in consecutive or serial order of both hands, with spaces for a lock of hair, information as to race, sex, and so on, that some ethnographical purpose might be served as well. At the close of 1879, and in January, 1880, I wrote out a hundred or so of circulars enclosing a number of my copperplate forms with outline hands to receive imprints. One copy made by me on 30th January, 1880, reads thus:—

"DEAR SIR,—I am at present engaged in a comparative study of the *rugae*, or skin furrows, of the hands of different races and would esteem it as a great favour if you should obtain for me nature-prints from the palmar surface of the fingers of any of the (blank) race in your vicinity, in accordance with the enclosed forms. The points of special interest are marked (red cross) and no others need specially be attended to. Each point must be printed by itself separately. Printer's ink put on very thinly and evenly, so as not to obliterate the furrows of the skin, is best. It can easily be removed by benzine or turpentine. In place of that, burnt cork mixed with *very little* oil will do very well. One or two trials had better be made before printing on the forms. If printing should be found too difficult, sketches of leading lines at the points indicated would still be of very great value, taking care that the directions corresponded with the furrows, and not in *reverse*, as when a simple impression is taken. If any one finger, and so on, comes out badly a piece of paper can be printed and pasted on at the proper place. I enclose as a specimen a filled-up form. [The fingers printed in the proper spaces and the important "points" each marked with a red cross.] As novel and valuable ethnological results are expected from this enquiry, I trust this may form a sufficient excuse for asking you to take so much trouble. Please return any forms which may be filled up to the above address.

"I am, &c.,

"H. FAULDS."

The response was quite disappointing. Some thought it was an advocate of palmistry looking for cats' paws; most took no notice whatever. I tried in the same way to get imprints from lemuroids, apes and anthropoids. On the 15th February of the same year (1880), I wrote to Charles Darwin, sending specimens of prints and an outline of my first results, and requesting him to aid me in obtaining access to imprints from lemurs, monkeys and anthropoids, as I had found them to show lineation patterns which I hoped might be serviceable for the elucidation of man's lineage. I had failed to find any trace of previous notices of the subject in anatomical or recent biological works. The few early notices which have yet been found I hope to deal with pretty fully on some other occasion. The great naturalist's reply, two years before his death, was as follows:—

The back view of a Japanese labourer most elaborately tattooed in colour.

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Via BRINDISI,
April 7th, 1880.

DOWN,
BECKENHAM, KENT.
RAILWAY STATION,
ORPINGTON, S.E.R.

DEAR SIR,—The subject to which you refer in your letter of February 15th, seems to me a curious one which may turn out interesting; but I am sorry to say that I am most unfortunately situated for offering you any assistance. I live in the country and from weak health seldom see anyone. I will, however, forward your letter to Mr. F. Galton, who is the most likely man that I can think of to take up the subject to make further enquiries.

Wishing you success,
I remain, dear Sir,
Yours faithfully,
(Signed) CHARLES DARWIN.

The original of the above holograph letter, with envelope addressed by Mr. Darwin and duly post-marked, along with the proof sheet of the first copperplate form made for me in Japan, is now in the Library of the Royal Faculty of Physicians and Surgeons, Glasgow. On October 28th, in *Nature* appeared a contribution by me, "On the Skin-furrows of the Hand," which was printed in the *Index Medicus* of the United States as the first recorded contribution on that subject. At the International Medical Congress about ten months afterwards, Dr. Billings, then editor of the *Index*, said in a speech—"Just as each individual is in some respects peculiar and unique, so that even the minute ridges and furrows at the end of his forefingers differ from those of all other forefingers, and it is sufficient to identify," and so on (Report in *The Times*, August 5th, 1881). My proposal was certainly the first public suggestion to establish a scientific method of identification on the basis of finger-prints. Sir William Herschel wrote soon afterwards to *Nature*, admitting my priority of publication, but stating that he had used a method of finger-prints in India before this. There is no dispute between Sir William Herschel and myself, as each had reached his own conclusions quite independently. This little personal matter was discussed in *Nature* (October, 1894) and in *Gegenbauer's Jahrbuch* for 1905, in which the date of my first contribution is considered the starting-point of recent study of the subject. The copious literature which soon sprung up was of every kind, with some appalling journalistic varieties in America. In 1881, Monsieur Bertillon, of Paris, brought out his delicate anthropometric system, to which the independent finger-print method from England was superadded. It therefore began to appear to the official, and even to the infallible encyclopaedic mind, that finger-prints were merely an element in the French system of identification, or Bertillonage. The finger-print method alone was used in a United States expedition in 1882, and it was tried in San Francisco, as afterwards in South Africa, to identify the fluctuating population of Chinamen. In the year after my final return to England greatly renewed interest was aroused in the subject. Herbert Spencer tried to explain the origin of the ridges in an article in *The Nineteenth Century*, May, 1886. Sir Francis Galton, to whom Charles Darwin wrote to me in 1880 that he would refer the matter, began the study, as he states on page 2 of "Finger Prints," in 1888. In that same year, Inspector Tunbridge from Scotland Yard was officially appointed to investigate my proposals. No report has ever been made public, but Mr. Tunbridge told me that he feared the method was too fine to work, and said that nothing could be done at least without fresh legislation. Some years afterwards he was appointed to New Zealand, where he was the means of inducing the prison authorities and police to apply the method, which has been now in successful operation all over Australasia for some years; so Mr. Tunbridge wrote to me in 1907.

In 1894, a committee appointed by Mr. Asquith met and finally, after some rambling conclusions, adopted Bertillonage with finger-prints as some help, the former being used as the basis of classification. The proposal was absurd, and it was soon found, as might have been foreseen, that finger-print patterns yield a far firmer and more searching basis of classification in themselves than the other method, and need no auxiliary crutches.

In 1897, the two associated methods began to be applied in British India; while in civil cases there, as in attestations, pension claims, and so on, the finger-print method was used by itself.

In 1901, the ten-finger method in serial order, exactly as originally advocated by me in 1880, was finally adopted in England, after other trials, and has met with an immediate and triumphant success in giving rapid and easy identifications of recidivists or old professional criminals, often living under aliases. Monsieur Bertillon, who at first did not use finger-prints at all, wrote to me officially that since 1894 the two methods had been jointly used in Paris, and that greater security was now felt in identifying. In 1902, finger-prints took the place of bodily measurements in Austria-Hungary, being easier of application, and less likely to give varying results. Two years afterwards Spain followed suit, our Inspector Arrow taking some charge. The method had been worked before that period in Buenos Ayres with success. A private service for identification by the finger-print method was, I believe, instituted in Belgium by Dr. de Laveleye, but I have heard no report of results.

It is a curious fact, but true beyond question, that the effectiveness of the method has proved to be the chief obstacle to its more extensive application. In short its miraculous effect in tearing the mask from old criminals who try to veil their identity by an *alias*, has created a horror of it amongst the class from which many recruits used to be drawn for Army and Navy.

In conclusion, I should like to point out that there are five distinct ways in which Dactylography, or the scientific study of finger furrows, may be serviceable:—

(1) In relation to the problem of human lineage. Much tentative work has been now done in this field

by many workers, and a scientific pathway begins to open up before us.

(2) In elucidating the relations of front and hind limbs; Professor Bowditch, of Harvard, wrote to me that he had early begun an enquiry into this subject. It promises to yield results of interest, but more workers are required.

(3) In identifying for life insurance, pensions, passports, affidavits, cheques, signing deeds, and so on. Again, in identifying the dead by former records, after battle, flood, fire or earthquake.

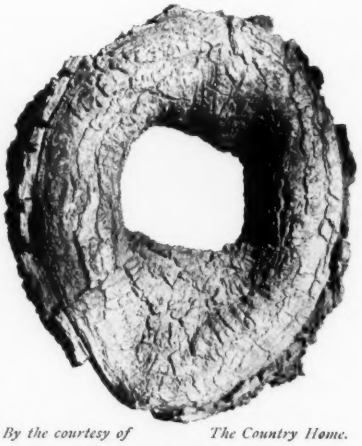
(4) In identifying old convicted criminals who have assumed other names.

(5) In testing evidence of bodily presence at a scene of crime by bloody finger-marks, sweaty or greasy smears on glass windows, wine glasses, lamps, or cash boxes and the like, or indented impressions on putty, wax, paraffin and so on. Faint impresses can be revived; invisible ones quite clearly brought out by chemical means; imprints *in relievo* may be photographed and made clearly intelligible to a jury. (See figure 5—a smudge from a finger.)

The last, and I think by far the least, of these once potential, now actual, utilities has taken the deepest hold of the popular imagination, and has seemed, to me at least, to threaten some danger to the innocent by its often ignorant and unscientific application. The method is not "mathematical" as certain officials are never tired of repeating, but demands common sense and the use of their own eyesight and mother wit by the plain men in the jury box. It is essentially English, and every accused person in the dock is as able as a judge, or counsel, or official witness, to test its validity.

WOUNDS IN TREES.

SOME interesting illustrations are given in the March number of *The Country Home* of wounds in trees that have successfully closed over, and others which will never properly heal. We are enabled to reproduce some of the pictures here. Figure 1 shows a well-healed wound. In Figure 2, the process of healing having been slow, the exposed wood has rotted away and, there being nothing to keep the healing tissues in their proper place, they are turning inward, with the result that if the wound is left to itself it will never heal. Figure 3 depicts a branch on which a snag has been left, and, so long as it remains, it will prevent the edges of the healing tissue from meeting. The article emphasises the need for more attention to be paid to trees, on account of the danger caused by their unexpected fall. Mention is also made of the work of the tree doctors, whose advertisements are seen in the columns of American newspapers. Their services would be useful at a time when the trees are lopped in the first instance, and also to repair the damage caused by ignorant labourers, who are only too commonly employed to cut trees, regardless of the fact that they are living creatures.



By the courtesy of *The Country Home.*

FIGURE 2. A wound that will not heal.

By the courtesy of *The Country Home.*

FIGURE 1. A well-healed wound.

By the courtesy of *The Country Home.*

FIGURE 3. A snag which prevents healing.

CORRESPONDENCE.

ORIGIN OF THE FINGER PRINT METHOD.

To the Editors of "KNOWLEDGE."

SIRS,—Kindly correct an error in your February number, Page 45, about Fingerprints. You quote Sir Francis Galton as saying in his Reminiscences "that Sir William Herschel in India had experimented with them since 1887." I do not know where you got this date from, but Galton never gave it. As a matter of fact before I left India in 1878 I had been experimenting for twenty years (Galton says "many years"), and had successfully established, not only the individuality, but the practical persistence of the pattern, as an unfailing method of identification, before any one else had entertained the idea. When I left India the system was in public use with the knowledge of the Heads of the Registration Department, and of Gaols, as well as for Pensioners, and in my own Court, and was matter of common knowledge in Bengal. The Government of Bengal had known of it from me many years before 1878, as a means of preventing forgery during the Indigo disputes in 1861 and 1862.

I trust you will do me the justice to insert this letter in your next number.

I am engaged at present in arranging my records for publication in facsimile by the Clarendon Press.

W. J. HERSCHEL.

A METEOR.

To the Editors of "KNOWLEDGE."

SIRS,—A beautiful meteor was seen here (Sunderland, February 16th) about 6.33 p.m. The head was almost as bright as Venus, but larger in appearance. It left a trail of light behind it which appeared to be reddish in hue; it was visible several seconds.

I am not an astronomer, but think that the note would be of interest to your readers.

F. H. S.

THE SUN'S REVOLUTION.

To the Editors of "KNOWLEDGE."

SIRS,—As a student of Astrological lore, I sometimes draw a figure for my birthday anniversary. But I find the time for the sun's return to its place at birth varies from year to year. For instance, at the time of my birth the sun was in Pisces 24·41, which occurred at 10 a.m. on March 15th, the year of my birth. But in 1910 the sun came to that point sixteen hours later, or at about 4 a.m. on the 16th, and this year later still, viz., about 9.30 a.m. on the 16th. Can any of your readers kindly explain how it is that the sun does not return to the same point in the Zodiac at the same time every year?

H. A. B.

IS SPACE INFINITE?

To the Editors of "KNOWLEDGE."

SIRS,—The following extracts from Professor Pickering's Article in *Popular Astronomy* of August last are of special interest in connection with Mr. Barclay's letter on the subject of the "Eternal Return." The suggestion that space is curved may be welcomed by many of your readers as an alternative theory to that of Laplace and others who have assumed that space and time are necessarily infinite.

H. PERIAM HAWKINS.

"Everyone who considers the question of proceeding indefinitely along a straight line must feel the impossibility of coming to a point where there is no space beyond him, and yet at the same time he must feel that infinite space is itself an impossibility. To avoid this difficulty it has been suggested that space is curved, with a definite radius and direction of curvature at every point. In other words we should accept the idea that plane triangles and rectangular co-ordinates are merely a close approximation to fact, but that all figures are really constructed in spherical co-ordinates.

"Now it is a property of infinite space, of any number of dimensions, that if it be properly curved, and inserted in space of one higher dimension, it will become finite. Thus if infinite space of one dimension, represented by a straight line, be properly curved and inserted in a plane it will become the periphery of an ellipse; if uniformly curved it will become that of a circle. Similarly, if space of two dimensions, represented by a plane, be properly curved and inserted in three-dimensional space, it will become the surface of an ellipsoid. Similarly, if ordinary or three-dimensional space be properly curved, and inserted in space of four dimensions, it will become finite in volume, and represent what would correspond to the surface of a fourth-dimensional solid. Thus, if we should go far enough east, we should reach the west, if far enough north we should reach the south, and if far enough into the zenith we should reach the nadir."

"If it is difficult for us to imagine infinite space, it is still more so to comprehend infinite time. As we go back eternally through the ages how is it possible for there to be still an infinity of time before that? Yet we cannot conceive of an actual day or instant before which time did not exist. In graphical solutions time is represented by a straight line, and may be compared to space of one dimension. But suppose that time, too, is curved, and has another dimension that we have not yet detected. Time then may be represented by an ellipse or a circle, and if we go back far enough into yesterday, we shall arrive at to-morrow. *Of course, we should not live our lives over again, because matter in the meantime would have changed, and when the present day again arrives, it will be upon a very different universe.* Both infinity of time and a return of time seem to us now impossible. If the latter is the more difficult to comprehend, may it not be simply because it has not occurred to us before?"

THE NEW MAP OF THE MOON.

To the Editors of "KNOWLEDGE."

SIRS,—Our nearest companion in space has, ever since the invention of the telescope, attracted a good deal of attention. Many have been the attempts to map its surface. Perhaps the first really useful map was that of Beer and Mädler, thirty-seven-and-a-quarter inches in diameter, published in 1834, those previously issued by Hevel and Tobias Mayer being much behind in detail, as also were those of Russell and Blunt. Lohrman's work was on the same scale as Beer and Mädler's, but, owing to the failure of his vision, was not completed until produced under the editorship of Schmidt, some thirty years ago. In 1876, Neison issued a useful book on the Moon, containing a capital map two feet in diameter. But in 1878, Schmidt's great map, six feet in diameter, was published, the labour of thirty-five years. Since these maps were issued a new era has dawned in selenography; the telescopist is reinforced by photography. The latter, whilst it misses the sharpness of telescopic vision, puts detail in its accurate place. The new map has been constructed from combined telescopic and photographic observations, on a scale of seventy-seven inches to the moon's diameter, by Mr. Walter Goodacre, the director of the Lunar Section of the British Astronomical Association. It has taken seven years to accomplish this noble work. Its accuracy may be judged when it is considered that it is based "mainly on 1,443 measured points on the lunar surface, made by Mr. S. A. Saunder, M.A., F.R.A.S., and published in the Memoirs of the Royal Astronomical Society, Volume LVII." The sketches of many of the best observers, including the late Major P. B. Molesworth and Mr. Goodacre, have been used in the construction. This monumental work, which has been exhibited

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both before the Royal Astronomical Society and the British Astronomical Association, is now being reduced to a scale of sixty inches, and divided into twenty-five sections. Each section is twelve inches square, with an overlap of half an inch, making the actual size thirteen inches. The map is undoubtedly the best one yet issued, and should prove a real incentive to the study of selenography. The fact that the moon's surface has been mapped far more accurately than has much of our own world does not alter the other fact, that there is very much more to learn concerning it. Mr. Goodacre has kindly given permission for the reproduction of one section (see Figure 1), and this shows how well he has accomplished his work. It is being produced by subscription at the really too low price of 22s. 6d., about a quarter the price of Schmidt's. It is to be hoped that the number of subscribers may very soon be found to permit of the publication being completed. FRANK C. DENNETT.

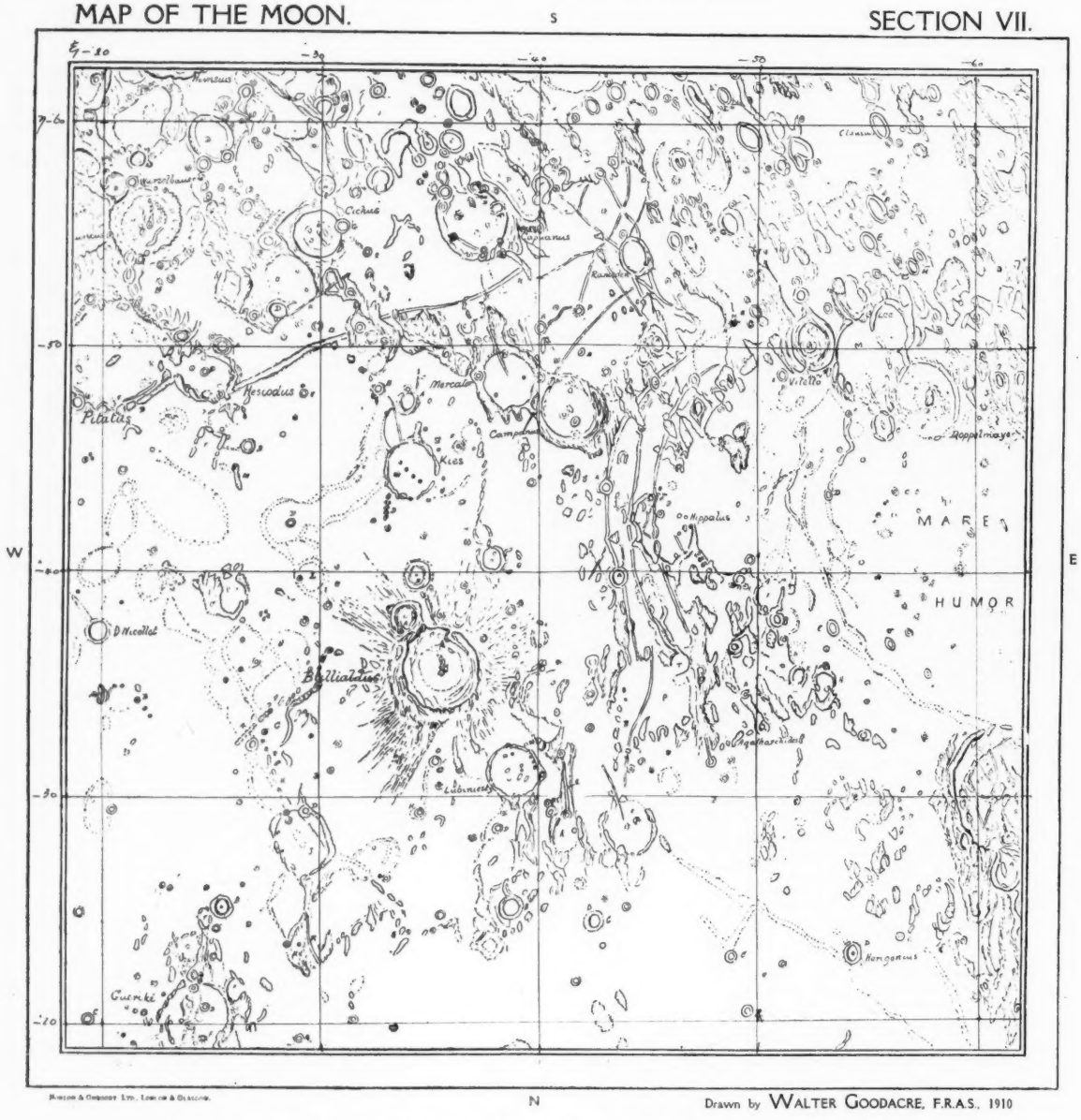

FIGURE 1. A reduced facsimile of one of the sections of a New Map of the Moon.

THE MOON AND THE WEATHER.

To the Editors of "KNOWLEDGE."

SIRS,—May I make a few notes on letters in your correspondence columns of July and September of last year, as a contribution towards discussion concerning the influence of the moon on the weather.

Mr. Strickland, in July—page 269—says: "Nasmyth believes it to be demonstrated that as a rule the sky is clearer at full moon than at the quarters." (If fewer clouds, then less rain; what do the rain-gauges say to this?) "Effect of moon is strongest in Winter in England." (Are tides highest in Winter?) "Equatorial regions are, *par excellence*, solar regions and lunar influence is reduced to a minimum." (What about height of tides there compared with temperate regions?)

Remarks on height of tide in Bay of Fundy, and in Mediterranean.—The almost absence of tide in the Mediterranean, is dependent upon the small inlet from the Atlantic,

and the high tides in other localized places upon the special configuration of the coast—the narrowing of the waterway, as also in the Severn and the Wye in England.

If the moon raises tides in the ocean, a much higher tide should she raise in the lighter air; if so, does her attraction counteract the effect of the increased weight of air on the barometer?

At the commencement of his letter Mr. Strickland says: "I hold that the negative they (the official meteorologists) think they have proved, is not proven." (Do not their statements rest on *facts* observed at Greenwich?)

In your September number,—page 372—Mr. Ditcham writes to show that the high tides in the Fraser River bring up warmer water from the sea, and so warm the climate, but is not that almost as indirect a result as it would be if they brought up a log of wood which he cut into firewood and warmed his house with? Or again, if the river were dammed back, the warming effect of the moon would be prevented to those above the dam. I live on the watershed between the Thames and the Channel where there are no tidal streams, and can find no sign of the moon's influence on our weather.

There is also a letter from Mr. Ditcham, in your February number of this year, the figures of which, I am sorry to say, I cannot understand, whether they give the height of tides, or some barometric weights, I do not know, nor can I see any connection between the figures and the dates. L. J.

PODURA SCALES.

To the Editors of "KNOWLEDGE."

SIRS,—Mr. Plaskitt, in his courteous reply to my letter, has almost entirely ignored the point I tried to drive home, that the available aperture of any wide apertured micro-objective is limited to the back lens being three-quarters filled with white light only. That I filled up the whole of mine with such light, I never asserted. Indeed, I could not have done so had I tried, my condenser being a dry one. For all that, I could always break down the image in my objective, of 1·40, upon any object mounted in balsam, by using the largest stop of the condenser. What I did claim, however, and do claim now, was that I worked the objective with the largest aperture it would stand—rather a different matter.

FIGURE 2.

Part of a Podura Scale, showing a portion of the right hand side of the Scale reproduced in "KNOWLEDGE," Volume XXXIII, page 333, Figure 2, enlarged 2½ times to show intermediate lines.

Upon the points he has raised, save one, I have nothing to fight; the laws of refraction are fixed. It is only upon the application we differ. Neither is it necessary on my part to try the experiments he suggests in the last paragraphs of his letter, for already I agree.

This question of full *versus* available aperture is a very old one with me—twenty years old, in fact. On referring to the back numbers of the journal of the Quekett Club, I find that Mr. Ingpen raised exactly the same point in 1891, in connection with some objects exhibited by myself. He said that he "wanted to employ the greatest powers of the objective to be obtained between 1 and 1·4, and did not see how they could be made use of upon a dry object,"—at the same time advocating the use of a dense medium.

To this, Mr. E. M. Nelson replied that he "found, on the other hand, if the object was dry on the cover glass, it would bear the test better than if in a medium. So long as it was in optical contact with the front of the lens they could get in all the spectra. . . . . Photographs of objects mounted in the denser medium looked all smeared over." He had never seen a decent critical image produced from anything in a dense medium. He did not know the reason, unless it might be that the treatment undergone by such might have the effect of spoiling them.

This appears to be a question of theory *versus* practice, and reminds me of the German Professor who forbade his students to adopt a certain formula, because, though it worked out well enough in practice, in theory it was all wrong. In the present instance the theory was that an oil-immersion objective must have an oil-immersion condenser of the same aperture to develop it. Workers bought the oil-immersion condensers and then innocently stopped them down until they got the working image, thinking all the while that they were utilising the full aperture of the lens. My challenge to them was to produce something under an oil-immersion condenser I could not show equally well with a dry one; a challenge never taken up. This, however, is very old history now.

I shall be only too pleased for the Editors to send Mr. Plaskitt my address, and equally pleased to receive the enlarged prints promised. I know how easy it is to miss little points in a small print which are perfectly obvious when they are further magnified. May I suggest, however, that with the Editors' permission, Mr. Plaskitt sends one enlargement to be reproduced for the benefit of the readers of "KNOWLEDGE." I am sending one with this letter hoping for that permission, because, I take it, the truth is what we both want. If this be granted, that is, the Editors' consent, the one of his to go best with mine would be the one with the oblique lighting across the scale.

Yet, even then, the credibility of appearances will still remain an outstanding question. I, at least, am not prepared to state with certainty what the secondary lines denote, and I do not suppose Mr. Plaskitt is more positive. We can only judge of the relative truth by the method of production. Leaving my own opinion out of the question altogether, the weighty authority of Mr. E. M. Nelson and the late Dr. Dallinger cannot be ignored, who both advocate the central cone of illumination, as opposed to oblique light. The last, in his presidential address to the Quekett Club, in 1891, speaking of the new apochromatism, says: "It gives certainty and precision to all work done . . . . but we must be careful not to re-introduce the ghostly element by false interpretation. I am increasingly convinced of the possible danger of employing shafts of oblique light only in one azimuth. The peril of misinterpretation is enormous."

Again, of the new apochromatic of 1·60 N.A., it is claimed that "it is a triumph of the optical firm which produced it. . . . . But I would hasten to say, that I would not trust a single result produced by its means, when oblique light in one azimuth is employed. . . . . It is fatal to its truth. We can absolutely get almost any desired result with it. It is a very optical Witch of Endor for calling up ghosts

and ghostly visions." Surely here is evidence enough, and as for this one lens, all the result of direct experiment.

Mr. Plaskitt contends that if when using an oil-immersion of 1·40 with the full aperture, that is, with the back lens filled with white light, I then stopped it down until only three-quarters was full, I should be getting much nearer ·75 than 1·40, an aperture not so much as a good dry quarter as to resolving power. Well, that is a matter easily settled. I still have the scale from which the photograph with my apochromatic was taken, and shall be most happy to wait upon him with it, if his home is in London. If he can then, with a dry glass, produce the same appearances upon it, I will concede his point. This, at least, would be a short way out of the discussion.

T. F. SMITH.

THE ETERNAL RETURN.

To the Editors of "KNOWLEDGE."

SIRS,—Mr. H. D. Barclay, in the February issue of "KNOWLEDGE," raises the oft-recurring and apparently insoluble question of Eternal Return, but the precise difficulty he is in is not altogether clear.

That time and space are infinite will admit of little doubt; that the sum total of forces in the Universe is also infinite is, though open to question, probably also true. The fact of their being constant need not necessarily impair their infinity. (Can infinity itself not be constant?) Their sum total may be constant, but their component parts are subject to perpetual changing and interchanging. It is this continual changing and lack of equilibrium that is of the very essence of force. Pre-suppose equilibrium, and our conception of force is gone. We might, indeed, say that this lack of equilibrium is a permanent and essential factor in the Universe. It is, as it were, the attribute of force or energy itself, and, in this sense, is infinite and eternal.

The argument, as stated by Mr. Barclay, pre-supposes this permanence:—"If these forces could ever attain a position of balance it would have already happened, as an infinity of time has passed." Not having happened, it follows that this condition of out-of-balance is infinite, and force, therefore, is also infinite.

It is curious to note that Dr. Le Bon is brought in as a supporter—indeed as one of the discoverers—of the eternal return hypothesis, but on what ground it is difficult to understand; for, in Dr. Le Bon's view, matter is constantly "dissociating" and slowly but surely returning to the ether from which it was originally derived. Once it has all returned, there is an end of matter and energy alike. This is a subject I happen to have alluded to in the January issue of the *Westminster Review*, and if Mr. Barclay will refer to it he will see that Dr. Le Bon's teaching (if I read him aright) is not that of "eternal return" but of "no return." It is true that here and there he lets fall a phrase which betrays a doubt on the subject; but in spite of these lapses it is clear that his doctrine of dissociation is closely bound up with the doctrine of "no return," — matter and energy are constantly being dissipated, and apparently are lost for all time. It is not an inspiring doctrine, and seems purely gratuitous; for there is nothing in his experiments that necessarily leads to any such inference, while the operations of Nature in general point altogether the other way. Nothing is clearer than that Nature, within the bounds of our experience, works, both on a large and on a small scale, in cycles of some sort. Almost all her operations are rhythmic. There is a constant ebb and flow: an evolution and a devolution. If this is the case in regard to phenomena within our limited experience, is it not a reasonable inference that this evolution and devolution extends far outside our experience and beyond our conception?—that it is, in fact, infinite and universal.

The following quotations from Dr. Le Bon's "The Evolution of Matter" (Book VI. Chapter VIII.) sufficiently indicate his views on the subject:—"We now know that matter vanishes slowly, and consequently is not destined to last for ever." . . . "What is the fate of the atom of electricity after the dissociation of matter? Is it eternal while matter is not?" . . . . "Once it [the electric atom] has radiated away all its energy, it vanishes into the ether and is no more." . . . . "This last, therefore, represents the final nirvana to which all things return after a more or less ephemeral existence."

One saving clause (in the same chapter) is as follows:—"Nothing leads to the belief that they [*i.e.*, things in general] had a real beginning or that they can have an end."

Thus we see that Dr. Le Bon's teaching is unmistakably in the direction of the final destruction of matter and energy, although the last quotation I have given betrays something of an open mind on the subject,—a subconscious admission though it possibly may be.

As often as we recur to this problem we are inevitably met with this consideration: that, from the nature of the case, the finite cannot grasp the infinite; and whatever our speculations may be, we must, of necessity, always labour under this disability. As Wallace has said: "Of infinity, in any of its aspects, we can really know nothing, but that it exists and is inconceivable."

This, however, is no reason for withholding our speculations, so long as they have a substantial basis of fact; for it is only by pressing them forward that we can ever hope to in any degree qualify our present limitations.

W. E. LISHMAN.

NOTICES.

THE ZOÖLOGICAL SOCIETY, ADDITIONS TO THE MENAGERIE.—During the month of February no less than one hundred and twenty-three additions to the Zoölogical Society's menagerie were registered. Among them are the following animals which are new to the collection:—A Dwarf Mongoose *(Helogale varia)*, from Mombasa, presented by the Rev. W. Douglas Braginton, on Feb. 27th; a Black-footed Polecat *(Putorius nigripes)*, from North America, received in exchange on Mar. 16th; two Dybowski's Deer *(Cervus hortulorum)*, from Manchuria, presented by Sir Edmund Loder, Bart., F.Z.S., on Feb. 23rd; and an Aldunati's Finch *(Phrygilus aldunatii)*, from Chili, presented by Miss Phillis True, on Feb. 13th.

ROYAL INSTITUTION.—The following are the Lecture Arrangements at the Royal Institution after Easter:—Mr. J. E. C. Bodley, Three Lectures on (1) Cardinal Manning; (2) The Decay of Idealism in France, and of Tradition in England; (3) The Institute of France; Professor Frederick W. Mott, Two Lectures on the Brain and the Hand; Professor W. W. Watts, Two Lectures on (1) The Ancient Volcano of Charnwood Forest (Leicestershire); (2) Charnwood Forest and its Fossil Landscape; Professor R. W. Wood, of the Johns Hopkins University, Three Lectures on the Optical Properties of Metallic Vapours (Illustrated); Dr. W. N. Shaw, Two Lectures on Air and the Flying Machine: (1) The Structure of the Atmosphere and the Texture of Air Currents; (2) Conditions of Safety for Floaters and Fliers; Mr. T. Thorne-Baker, Two Lectures on (1) Changes effected by Light; (2) Practical Progress in Wireless Telegraphy (Illustrated); Professor Selwyn Image, Three Lectures on (1) John Ruskin; or, the Seer and Art; (2) William Morris; or, the Craftsman and Art; (3) Walter Pater; or, the Connoisseur and Art; Mr. W. P. Pycraft, Two Lectures on Phases of Bird Life: (1) Flight; (2) Migration; and Mr. W. L. Courtney, Two Lectures on Types of Greek Women; Nausicaa and the Homeric Women; Sappho and the Aeolian Poets; Aspasia and Pericles. The Friday Evening Meetings will be resumed on April 28th, when a Discourse will be given by Professor W. M. Flinders Petrie on The Revolutions of Civilization. Succeeding Discourses will probably be given by Professor Martin O. Foster, Professor William Stirling, Professor R. W. Wood, Professor Gilbert Murray, Commendatore G. Marconi, Professor Svante Arrhenius, and other gentlemen.

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THE FACE OF THE SKY FOR APRIL.

By W. SHACKLETON, F.R.A.S., A.R.C.S.

THE SUN.—On the 1st the Sun rises at 5.39, and sets at 6.29; on the 30th he rises at 4.37, and sets at 7.17. The equation of time is negligible on the 16th and 17th, hence these are convenient days for the adjustment of sundials, as only the longitude correction is needed. Sun-spots may usually be seen on the solar disc, but they are small, and not numerous. The positions of the Sun's axis, equator, and heliographic longitude of the centre of disc are shown in the following table:—

Date.	Axis inclined from N. point.	Centre of Disc S. of Sun's Equator.	Heliographic Longitude of Centre of Disc.
Apl. 1 ...	26° 18′W	6° 31′	188° 56′
,, 6 ...	26° 26′W	6° 13′	122° 58′
,, 11 ...	26° 23′W	5° 52′	56° 58′
,, 16 ...	26° 10′W	5° 29′	350° 56′
,, 21 ...	25° 45′W	5° 3′	284° 54′
,, 26 ...	25° 9′W	4° 35′	218° 51′
May 1	24° 22′W	4° 6′	152° 46′
,, 6 ...	23° 24′W	3° 35′	86° 41′

On April 28th, a total eclipse of the Sun takes place. It is invisible in this country; the path of totality lies almost entirely over the Pacific Ocean, no large piece of land falling in the shadow. A party of observers left England in February for Vav-au, one of the islands in the Tonga or Friendly Group, in Mid-Pacific, where totality lasts for about three-and-a-half minutes.

THE MOON:—

Date.	Phases.	H. M.
Apl. 6 ...	☽ First Quarter	5 55 a.m.
,, 13 ...	○ Full Moon	2 37 p.m.
,, 21 ...	☾ Last Quarter	6 36 p.m.
,, 28 ...	● New Moon	10 25 p.m.
May 5 ...	☽ First Quarter	1 14 p.m.
Apl. 2 ...	Perigee	8 12 a.m.
,, 18 ...	Apogee	6 42 a.m.
,, 30 ...	Perigee	9 0 a.m.

OCCULTATIONS.—No bright stars are occulted before midnight either in April or May.

THE PLANETS.

MERCURY:—

Date.	Right Ascension.	Declination.
	h. m.	
Apl. 1 ...	1 24	N 9° 25′
,, 11 ...	2 25	16° 52′
,, 21 ...	2 58	19° 55′
May 1 ..	2 55	18° 11′
,, 11 ...	2 35	N 13° 48′

Mercury is an evening star throughout April. The planet is at greatest Easterly elongation from the Sun of 19° 42′ on the 15th, when he sets W.N.W. about 8.55 p.m. This elongation is a favourable one on account of the high declination, and there should be no difficulty in seeing the planet from the 15th to the 25th, as he sets about two hours after the Sun.

On May 5th, Mercury is in inferior conjunction with the Sun.

VENUS:—

Date.	Right Ascension.	Declination.
	h. m.	
Apl. 1 ...	2 31	N 15° 10′
,, 11 ...	3 19	19° 7′
,, 21 ...	4 9	22° 15′
May 1 ..	5 0	24° 25′
,, 11 ...	5 51	N 25° 29′

Venus is a brilliant object in the evening sky, looking W.N.W. after sunset.

The planet is well placed for observation, appearing fairly high above the horizon at sunset, and not setting till 9.20 p.m. on April 1st, and 10.50 p.m. on May 1st.

As seen through the telescope the planet appears gibbous, 0·8 of the disc being illuminated, whilst the apparent diameter of the disc is 13″. On account of her brightness the planet is fairly easy to pick up with the naked eye before it is dark, and this is the best time for making observations with the telescope, for Venus is a severe test for the achromatic qualities of any refractor if observed on the dark background of the sky; seen in a lighter sky the outstanding colour, in the telescope, due to want of perfect achromatism, is not so obtrusive, and a better view of the planet is obtained. The Moon will appear near the planet on April 1st, being only 0° 14′ to the South as illustrated in the March issue, whilst on May 1st the Moon will be 1½° to the North.

MARS:—

Date.	Right Ascension.	Declination.
	h. m.	
Apl. 1 ...	21 6	S 17° 51′
,, 11 ...	21 36	15° 42′
,, 21 ...	22 5	13° 19′
May 1 ...	22 34	10° 45′
,, 11 ...	23 2	S 8° 3′

Mars is a morning star rising E.S.E. about 3.30 a.m., near the middle of the month. He is situated first in Capricorn, then in Aquarius. The planet is unfavourably placed for observation and since his apparent diameter is only 6″ telescopic observations are difficult except in large instruments.

JUPITER:—

Date.	Right Ascension.	Declination.
	h. m.	
Apl. 1 ..	14 44	S 14° 28′
,, 11 ...	14 40	14° 9′
,, 21 ...	14 36	13° 47′
May 1 ...	14 31	13° 23′
,, 11 ...	14 26	S 13° 0′

Jupiter is a brilliant object in the late evening sky looking South-East. The planet is in opposition to the Sun on May 1st, hence about this date he appears South at midnight. On April 1st the planet rises at 9.20 p.m., and on May 1st at 7 p.m., and is visible throughout the night. The equatorial diameter on the 15th is 43″·6, whilst the polar diameter is 2″·8 smaller; this polar flattening is a conspicuous feature of Jupiter, and is readily observable in small telescopes; also the

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attendant bright moons and belt markings on the planet's disc form interesting objects of observation. The smallest telescope magnifying about 40 times, shows the planet of the same apparent diameter as the Moon seen with the naked eye, and the surface markings may readily be observed.

The following table gives the satellite phenomena visible before midnight :—

Date.	Satellite.	Phenomenon.	P.M.'s. H. M.	Date.	Satellite.	Phenomenon.	P.M.'s. H. M.	Date.	Satellite.	Phenomenon.	P.M.'s. H. M.
Apl.				Apl.				Apl.			
4	III.	Tr. E.	9 53	15	I.	Ec. D.	10 42	23	I.	Sh. E.	11 57
7	I.	Sh. I.	11 27	16	I.	Sh. E.	10 3	24	I.	Oc. R.	9 22
8	I.	Oc. R.	11 28	16	I.	Tr. E.	10 22	29	III.	Ec. D.	8 27
11	III.	Sh. I.	10 15	22	II.	Sh. I.	9 35	29	III.	Ec. R.	9 49
11	III.	Sh. E.	11 56	22	II	Tr. I.	10 1	30	I.	Sh. I.	11 39
15	II.	Sh. E.	9 39	23	I.	Sh. I.	9 45	30	I.	Tr. I.	11 49
15	II.	Tr. E.	10 18	23	I.	Tr. I.	9 56				

"Oc. D." denotes the disappearance of the Satellite behind the disc, and "Oc. R." its reappearance; "Tr. I." the ingress of a transit across the disc, and "Tr. E." its egress; "Sh. I." the ingress of a transit of the shadow across the disc, and "Sh. E." its egress; "Ec. D." denotes disappearance of Satellite by Eclipse, and "Ec. R." its reappearance.

SATURN :—

Date.	Right Ascension.	Declination.
	h. m.	
Apl. 1 ...	2 18	N 11° 28′
,, 16 ...	2 25	12° 6′
May 1 ...	2 32	N 12° 43′

Saturn is observable for a few evenings during the early part of the month, when he appears in the W., between Venus and the horizon. During the greater part of the month the planet appears in too bright a portion of the sky to be observed. On May 1st the planet is in conjunction with the Sun and hence will be unobservable for some weeks following.

URANUS :—

Date.	Right Ascension.	Declination.
	h. m. s.	
Apl. 1 ...	20 4 34	S 20° 53′ 48″
May 1 ...	20 6 29	S 20° 49′ 7″

Uranus is visible in the mornings, rising at 3.18 on April 1st and 1.21 on May 1st. The planet is situated about 2° S.E. of σ Capricorni; he is in quadrature on the 20th April and at the stationary point on the 4th May.

NEPTUNE :—

Date.	Right Ascension.	Declination.
	h. m. s.	
Apl. 1 ...	7 20 55	N 21° 31′ 41″
May 1 ...	7 22 5	N 21° 30′ 19″

Neptune is on the meridian at 6.45 p.m. on April 1st and at 4.47 p.m. on May 1st, and sets at 2.46 a.m. and 0.49 a.m. on these dates. The planet is situated in Gemini about $3\frac{1}{2}$ degrees S.E. of the star δ Geminorum.

METEOR SHOWERS :—

Date.	Radiant. R.A.	Radiant. Dec.	Name.	Characteristics.
	h. m.			
Apr. 17–May 1	16 0	+47°	τ Herculids	Small; short.
,, 20–21 ...	17 24	+36°	π Herculids	Swift; bl. white
,, 20–22 ...	18 4	+33°	*Lyrid Shower*	Swift.
,, 30 ...	19 24	+59°	o Draconids	Rather slow.

Algol may be observed at minimum on the 3rd at 8.52 p.m. and on the 23rd at 10.35 p.m. Its period is $2^d\ 20^h\ 49^m$, from which other minima may be deduced.

TELESCOPIC OBJECTS :—

DOUBLE STARS.—γ Virginis, XII.^{h} 37^m, S. 0° 54′, mags. 3, 3; separation, 6″·0. Binary system; both components are yellow, though one is of a deeper hue than the other. An eyepiece of magnifying power of 30 or 40 is required on a 3-in. to effect separation.

π Boötis, XIV.^{h} 36^m, N. 16° 53′, mags. 4, 6; separation 6″. Requires a power of about 40.

ϵ Boötis, XIV.^{h} 41^m, N. 27° 30′, mags. 3, $6\frac{1}{2}$; separation 2″·6. Very pretty double, with good colour contrast, the brighter component being yellow, the other blue-green.

ξ Boötis, XIV.^{h} 47^m, N. 19° 31′, mags. 5, 7; separation 2″·4. Binary; one component being orange, the other purple.

CLUSTERS.—M 3 (*Canes Venatici*) XIII.^{h} 38^m, N. 28° 48′. This object, though really a globular cluster of myriads of small stars, appears more like a nebula in small telescopes. It is situated between Cor Caroli and Arcturus, but rather nearer the latter.

NOTICES.

PLASKITT'S SIMPLEX CALCULATOR.—We have received from Mr. F. W. K. Plaskitt, F.R.M.S., a copy of a series of tables, printed on a handy card, for finding the day of the week of any particular date. The tables are easy to work with, the amount of calculation is small, and so far as the results have been tested they have always been correct. The price is 6½d. post free, from the Author at 12, Woodbeech Street, E.C.

APPARATUS FOR USE IN PLANT PHYSIOLOGY.—Our botanical readers will be interested to know that Messrs. Bausch and Lomb are now able to supply sixteen important pieces of apparatus from among those which have been designed by Professor Ganong, of Smith College. To a new catalogue of these, Professor Ganong contributes an introduction, in which he says that it diverts time and energy from the phenomena of the plant to centre them on a somewhat slovenly kind of mechanics, and a wholly wrong ideal is inculcated of the nature of scientific work which is based on precision, logic and quantation. What is there peculiar in plant physiology, he asks, that in it alone of all the sciences it is better to do imperfect work with self-made tools than to exact work with good tools made expressly for the purpose.

The apparatus includes a clinostat, a photosynthometer, light screens, potometers, and smaller but equally useful contrivances.

The list can be obtained on application to Messrs. Bausch and Lomb's London address, 19, Thavies Inn, Holborn Circus, E.C.

MESSRS. NEWTON & CO.'S NEW CATALOGUE.—We have received a copy of Messrs. Newton & Co.'s new catalogue of X-Ray and Electro-Medical Apparatus which runs into 167 pages. By an ingenious arrangement it is possible to turn at once to any section of the catalogue, whether it be that concerned with X-Ray work, or apparatus dealing with medical electrical currents, light, or high frequency.

THE MICROSCOPE AND SOME HINTS ON HOW TO USE IT.—Mr. E. Leitz has issued a useful little pamphlet under the title given in our side-heading. It should be especially useful to beginners, as it describes the parts of a microscope in detail and explains the terms such as aperture, resolving power and so on, which are applied to it. There are paragraphs on such topics as focussing, and the making of measurements under the microscope, while three pages are devoted to general hints as to the treatment and care of the instrument.

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NOTES.

ASTRONOMY.

By A. C. D. Crommelin, D.Sc.

DIAMETERS OF STARS.—The question of the diameters of the stars is at once a most interesting and a most difficult one. Both the late Mr. R. A. Proctor and Major Macmahon have suggested using occultations of stars by the Moon for this purpose, the former by putting the star image into rapid rotation and noting the arc over which fading extended, the latter by trying to record the disappearance on a photograph of large scale. However, it is improbable that either of these schemes is workable, and they would be limited to stars near the Ecliptic. M. Charles Nordmann has attacked the problem in another manner, (*Comptes Rendus de l'Académie*, 1911, January 9th.) It is clear that the problem would be soluble if we knew the distance of a star, its brightness compared with the Sun's at an equal distance and the ratio of its surface brightness to that of the Sun. The first datum is known with tolerable accuracy for the nearer stars; the second follows from it; the third is the most difficult, but M. Nordmann has given a formula for the "intrinsic effective brightness" based on a study of the relative intensity of different positions of the visible spectrum. (Thus it is generally admitted that the stars with the Sirian type of spectrum have a greater surface brilliance than those with the solar type, and still more than those of a decided red colour). This is obviously the most open to doubt of the assumptions, but we may take M. Nordmann's list as giving at least a rough idea of the dimensions of some of our stellar neighbours. He takes the Sun's stellar magnitude as $-26{\cdot}83$, which is the result of recent Harvard measures and is in good accord with the mean of the best previous results.

Star.	Mag.	Assumed Parallax.	Effective Temperature.	Diameter (Sun's = 1)
Sirius ...	$-1{\cdot}58$	$0{\cdot}''37$	12,200° (Cent.)	$1{\cdot}13$
Procyon ...	$0{\cdot}48$	$0{\cdot}''30$	6,810° ,,	$1{\cdot}35$
Aldebaran ...	$1{\cdot}06$	$0{\cdot}''15$	3,500° ,,	$13{\cdot}50$
Capella ...	$0{\cdot}21$	$0{\cdot}''12$	4,720° ,,	$8{\cdot}26$
Vega	$0{\cdot}14$	$0{\cdot}''12$	12,200° ,,	$1{\cdot}57$
γ Cygni ...	$2{\cdot}32$	$0{\cdot}''10$	5,620° ,,	$0{\cdot}92$
α Persei ...	$1{\cdot}90$	$0{\cdot}''08$	8,300° ,,	$1{\cdot}83$
Polaris ...	$2{\cdot}12$	$0{\cdot}''07$	8,200° ,,	$1{\cdot}93$
β Andromedae	$2{\cdot}37$	$0{\cdot}''07$	3,700° ,,	$13{\cdot}0$
β Persei (Algol)	$2{\cdot}10$	$0{\cdot}''05$	13,800° ,,	$1{\cdot}29$

It will be seen that Aldebaran and β Andromedae are the giants of the list, the result for the former being entitled to more weight owing to the greater parallax. The above value would make the apparent diameter of Aldebaran $0{\cdot}''018$, which, as M. Nordmann points out, would require an aperture of twenty feet for its direct measurement. A direct occultation of this star by the Moon would occupy one-thirtieth of a second, which might be increased two or three times or even more, in an oblique occultation. It will be seen that, according to the figures, Aldebaran surpasses our Sun in size more than our Sun does Jupiter. The likelihood that Sirius does not greatly surpass the sun in size, in spite of its great brilliance, was already inferred from the fact that its mass is only double his, so the new value is quite reasonable. Arcturus, not included in this list, is another great sun; Miss Agnes Clerke said of it:—"Perhaps the most stupendous orb within our imperfect cognisance."

THE SYSTEM OF ALGOL.—Algol, the last star on the list, was the first to have its diameter measured by Vogel using the spectroscopic method to find the rate of motion; assuming the densities of Algol and the companion to be equal, he found 1,061,000 miles for the diameter, or $1{\cdot}2$ of the sum as a result, independent of the parallax, and curiously close to that independently found by M. Nordmann. A new and beautiful investigation of this system has recently been published by Mr. Joel Stebbins, of the University of Illinois, (*Astrophys Journ.*, October, 1910). He uses a selenium photometer attached to a twelve-inch refractor. An out-of-focus image of the star, seven millimetres in diameter, is thrown for ten seconds on a selenium plate, which is in an electric circuit forming one arm of a Wheatstone bridge; the galvanometer deflection is measured, and corresponding readings taken on α Persei and δ Persei for comparison. The method is far more sensitive than that of visual comparisons, and reveals (what had long been visually sought in vain) a secondary minimum when the companion is occulted by Algol, the depression being $0{\cdot}06$ magnitude. He infers that, taking the parallax $0{\cdot}''05$, the side of the companion nearest Algol gives six times the light of the Sun, and the other side three times. The difference is inferred from the fact that a continuous increase in light is noted from principal minimum to secondary minimum, and the probable explanation is the tremendous radiation from Algol to which it is subjected. On different assumptions as to the distribution of density in the system, the estimates of the diameter of Algol range from $0{\cdot}81$ to $1{\cdot}45$ of the Sun, the companion being one-seventh larger than the primary, and the eclipses partial; the apparent ellipse being about 8° open. On all the hypotheses the surface brilliancy of Algol is much greater and its density much less than those of the Sun.

Mr. Stebbins is to be congratulated on his successful application of the selenium cell, which will doubtless in the future be largely employed for measuring small light changes. The idea is not new, but previous attempts have not met with equal success. One necessary precaution is to enclose the cell in an ice-box, its sensitiveness being greatly increased.

THE EIGHTH SATELLITE OF JUPITER.—This tiny body will be out of reach at Greenwich for some years, while Jupiter is south of the Equator; it is therefore a matter of satisfaction that it was photographed during February with the large reflector at Helwan in Egypt. The positions are within some 20″ of those predicted. The error can be removed by increasing the adopted period (738·9 days) by a quarter of a day, and making a very slight increase in the eccentricity. This satellite will ultimately give a very accurate mass of Jupiter, but it must be observed over a larger arc before it can be fully utilised for this purpose.

ADOPTION OF WESTERN EUROPEAN (GREENWICH) TIME IN FRANCE.—This adoption took effect on March 10th, the date of the meeting of the Royal Astronomical Society, who passed a resolution authorising their President to despatch a telegram of congratulation to the President of the French Republic. An important advance is thus made towards the attainment of the ideal universal time, the minutes to be the same everywhere, while the hours change in each zone of 15° of longitude.

BOTANY.

By Professor F. Cavers, D.Sc., F.L.S.

SOME RECENT WORK ON GEOTROPISM.—Giltay (*Zeitschr. f. Botanik, 1910*) has recently discussed and criticised various methods of experimentation on the problems of geotropism, and has called attention to a point which is often overlooked by students of Vegetable Physiology, namely, that we have never proved that gravitation is the *only* stimulus involved in the turning of the primary root towards the centre of the earth. Long ago, Knight (1806) showed that as the centrifugal force was increased on a centrifuge with a vertical axis, the root and stem assumed a more nearly horizontal

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position, but he did not show any relation between the position of stem and root and the resultant of the two forces involved—gravity and centrifugal force; that is, Knight showed that at least in part the so-called geotropic stimulus is the gravity stimulus, but he did not show that the gravity stimulus is the only stimulus involved. Giltay devised a special centrifuge in order to test whether the position taken up by the root and stem is in reality the resultant of the two forces, and found that this is the case, with slight deviations that could be accounted for by variations in the speed of rotation and the variation of the roots themselves. Hence it may be assumed that the geotropic stimulus is identical in Nature with the gravity stimulus and with that of centrifugal force.

Since 1880, when the Darwins published ("Power of Movement in Plants") the results of their experiments on the behaviour of decapitated roots, there has been a good deal of controversy regarding the perceptive region of the root. Czapek (*Jahrb. wiss. Bot.*, 1895) caused the root-tip of seedlings to grow into a boot-shaped glass cap, and showed that when the terminal portion (1.5 millimetres long) of the capped root was placed horizontally, the portion outside of the "boot" curved so as to bring the root-tip into the position of equilibrium—the vertical position. On the other hand, if the seedling was fixed so that the tip was vertical and the rest of the root horizontal, no curvature took place, the root simply continuing to grow without changing the position of the tip or of the elongating zone. These results were interpreted to mean that only the apical one or two millimetres of the root was sensitive to gravitation, and it was generally thought that the matter had been finally settled. The simple experiment of cutting off two millimetres of the root tip, though it destroyed the sensitiveness of the root, had been objected to on the ground that the wounding might have also destroyed the sensitiveness of the elongating zone behind the wound, but Czapek's ingenious experiment was regarded as final, though Jost and a few other writers have steadily maintained that the question is still open. Newcombe (*Beih. Bot. Centralb., Band xxiv., Abt. i.*), after upholding these objections and pointing out that "neither Czapek's nor any other method so far employed has or can prove the restriction of the perceptive region to the apical two millimetres of the root," proceeds to show (1) that all the phenomena observed accord equally well with the view that sensitiveness extends through the entire growing zone, but becomes diminished from the apex backwards, or the view that sensitiveness is uniform through the growing zone, but the tendency to automatic curvature—autotropism—is stronger in the hinder than in the apical region; (2) experiments on the centrifuge with decapitated roots show that geotropic sensitiveness is present more than four millimetres distant from the tip. It must be admitted, of course, that in a question of this kind the same results may be interpreted in diametrically opposite ways by different observers, but it would certainly appear that we should keep an open mind on the matter. Newcombe's paper is of importance as a reminder that even the most fundamental questions relating to reflex actions in plants are by no means settled yet, and that much further work is necessary before it will be possible to obtain a clear picture of what happens in the growing tip of a root when it emerges from the seed and grows down into the soil.

Another aspect of geotropism has just been re-investigated by Nienburg (*Flora, Band 102*, 1911) in connection with the movements of twining stems. Nienburg's work, like that of Newcombe on the root, was done largely with centrifugal apparatus, and he also has obtained results at variance with those of previous investigators. In 1881, it was shown by Schwendener that the rotating movements of twiners like Hop or Convolvulus, are not made by the stem when the plant is kept revolving on a klinostat, and later it was suggested by Noll that twining stems make a peculiar response to the stimulus of gravitation, in that growth is promoted on one flank, instead of on the upper side as in a root, or the lower side as in an ordinary stem. This response was termed "lateral geotropism," its result being a revolving motion of the shoot apex. The clasping of supports by tendrils, like those of Peas or Vines, is, of course, an entirely different phenomenon, due to contact irritability. From his experiments, Nienburg concludes that all the facts observed in the growth of twining plants can be explained as due to the combined action of autonomous rotation (nutation) and ordinary negative geotropism, and that neither Noll's experiments nor those of later writers have established the existence of any such thing as "lateral geotropism." However, it would appear that here again we have an as yet unsettled question, and one which requires further investigation with the aid of klinostat, centrifuge, and other methods of experimentation.

The curious "peg" or "heel" that grows out from the seedling of Cucumber, Marrow, and other Cucurbitaceae, and which holds down the seed-coat and helps the young shoot to escape, has been recently investigated carefully by Crocker, Knight, and Roberts (*Bot. Gaz.*, November, 1910). According to Francis Darwin, Cucurbit seedlings allowed to germinate on a slowly-rotating klinostat produce pegs completely surrounding the young shoot, and therefore appearing like a collar, and he concluded that gravity determines the lateral development of the peg, and that therefore this experiment shows that gravity is continually effective on the klinostat and is simply equalised in its action on the several flanks of the rotated object; he also used the peg as a support for the memory theory of plant response, assuming that its development and position are directly determined by gravity. Crocker, Knight, and Roberts, as the result of many experiments, conclude that there is no evidence that gravity acts as a direct stimulus to the lateral development of the peg, or that it leads to increase in size of the peg; that if the young shoot (hypocotyl) is prevented from arching, the peg develops equally all round; that the lateral development of the peg is simply brought about by the arching of the hypocotyl, the most effective factor in this arching being the contact of the seed-coat.

CITRIC FERMENTATION.—Since Wehmer in 1893 described the process of citric fermentation and showed that it is due to a mould-like fungus, *Citromyces*, several workers have dealt with the subject, and Wehmer has recently given a critical summary of their papers and of his own further researches on citric acid fermentation, and the Citromycetes (*Zeitschr. f. Bot., Heft 2*, 1911). The process is evidently in the main one of oxidation of sugar—especially of malt, cane, and grape sugars—and under favourable circumstances as much as fifty per cent. of the sugar may be converted into citric acid. The three sugars named yield the largest proportion of the acid when acted on by the fungus, but a fair yield is obtained from glycerine (nearly thirty per cent.), and much smaller quantities or merely traces from various sugars, inulin, alcohols. Up to the present it is doubtful whether the action is caused by an enzyme produced by the fungus, for negative results have been obtained with expressed sap and with killed fungus. Many interesting questions regarding the mode of action of the citric acid fungus arise from recent investigations. It has been suggested that since acid is formed, though sparingly, in the absence of free oxygen, the first step in the action must be the splitting of the sugar into carbon dioxide and alcohol—as in alcoholic fermentation. The alcohol would then be oxidised with the production of citric acid, this second action or citric fermentation proper corresponding with the analogous processes of lactic and acetic fermentation.

RECENT WORK ON THE GNETALES.—The remarkable group Gnetales includes the three genera *Gnetum*, *Ephedra* and *Welwitschia*, which differ from all other Gymnosperms in having compound inflorescences, a long micropylar tube, and true vessels, and which make a further approach to the Angiosperms in the fact that except in *Ephedra* the archegonia are reduced to isolated cells. Various writers have suggested that the Gnetales form a transitional group between Gymnosperms and Angiosperms, though it seems more likely that they had a common ancestry with the Angiosperms, and developed parallel with them. Porsch (*Ber. deutsch. bot. Ges.*, 1910)

has shewn that *Ephedra campylopoda*, found in the south of Europe, is not only adapted for insect-pollination, but is regularly visited by insects. In his interesting memoir on this the first established case of insect pollination among the Gymnosperms, Porsch shows that the yellowish-red colour of the inflorescences attracts insects, which feed on the sticky pollen, and that nectar is provided in the form of drops which ooze from the micropyle. The insect-visitors included thirteen species of Hymenoptera and Diptera, which were found to carry the pollen on the underside of their bodies. Since secretion of sugary liquid occurs in the flowers of *Gnetum* and *Welwitschia*, it is very likely that these genera also are insect-pollinated, though, until now, it has been supposed that the function of the liquid which oozes from the micropyle, is simply that of catching pollen blown by the wind, in much the same way as in the female cones of pines. Pearson (*Phil. Trans. Roy. Soc.*, 1909) had already suggested that the flowers of *Welwitschia* are insect-pollinated, and in this plant there are glandular outgrowths below the anthers which doubtless act as honey-glands. Pearson has also described, though not fully, the development of the ovule of this remarkable genus. The endosperm begins, as usual, in Gymnosperms, with free nuclear division, forming an embryo-sac with approximately one thousand and twenty-four free nuclei, representing ten successive divisions. These nuclei are distributed through the sac, so that when cell-walls are formed the sac is divided into multinucleate cells; those in the micropylar (upper) region contain fewer nuclei than the others, and they become the nuclei of free eggs. The cells of the lower three-fourths of the endosperm contain many nuclei, and these fuse and form a uninucleate tissue—the primary endosperm, which continues to grow both before and after fertilisation. The multinucleate cells of the micropylar region send out tubes into the overlying nucellar tissue, into which the free nuclei pass, and these prothallial tubes meet the pollen-tubes in the lower half of the nucellar cap.

CHEMISTRY.

By C. AINSWORTH MITCHELL, B.A. (Oxon.), F.I.C.

COMBUSTION OF GASES WITHOUT FLAME.—A new property of copper is described by M. J. Meunier in the *Comptes Rendus* (1911, clii, 194). On heating a wire of pure copper in the luminous flame of a Bunsen burner until all surface oxide has been reduced to metallic copper, and then admitting air the wire will begin to glow. On now lowering the hot copper into the tube of the burner, the steady glow will continue without igniting the mixture of air and gas in the tube. The maximum intensity of glow is obtained when the proportion of gas in the mixture is about 30 per cent. During this slow combustion the copper is rendered exceedingly brittle and may readily be reduced to a powder of crystalline appearance.

CELTIUM: A NEW ELEMENT.—M. G. Urbain describes in the *Comptes Rendus* (1911, clii, 141), a new element for which he suggests the name *Celtium* and the symbol Ct. It was found accompanying the elements lutecium and scandium in gadolinite earth, and was isolated from the mother liquor obtained in the separation of lutecium. In its general properties it is intermediate between these two metals, and differs from both in its magnetic permeability and in its spectrum. Its chloride is more volatile than that of lutecium, but less volatile than that of scandium, while its hydroxide is a stronger base than scandium hydroxide, but is weaker than lutecium hydroxide. Its atomic weight has not yet been determined.

HELIUM IN THE AIR OF VESUVIUS.—In the only recently published proceedings of Section II of the Seventh International Congress of Applied Chemistry of 1909 (pages 83–86), Mr. A. Piutti gives an account of his investigation of different incrustations from Vesuvius which at various periods he has tested for helium. In each case the substance was heated and the gas emitted was absorbed by cooled charcoal. A specimen of sanidinite from Vesuvius yielded 0·106 c.c. of gas per gramme. Its radio-activity was found to be principally due to the presence of zircon crystals, and these, like the gas, contained helium.

In like manner a specimen of pink tourmaline, which yielded 0·511 c.c. of gas per gramme was found to contain helium, and this element was also identified in the air of Naples as well as of Vesuvius.

Its presence was also proved in the zircons from many other localities, and in the case of large tourmalines from Madagascar the green outer portion was more radioactive and contained more helium than the inner light pink part. In general, though not invariably so, the proportion of helium corresponded with the degree of radio-activity.

SPONTANEOUS IGNITION OF COAL.—The results of a bacteriological investigation of the spontaneous combustion of coal, by Mr. E. Galle, an outline of which is given in the *Chem. Zentralbl.* (1911, I. 48), have suggested several interesting conclusions. Cultivations were made both in the presence and absence of air, and seven species of bacteria were isolated from coal. Of these, four species (*B. nacraceus*, *B. subtilis*, *B. mesentericus*, and *B. pseudosubtilis*) were found to be capable, when grown on suitable nutrient media in the presence of coal dust, of producing combustible mixtures of gas containing from 5·4 to 27·3 per cent. of carbon dioxide, and 71·5 to 84·8 per cent. of methane, together with traces (less than three per cent.) of carbon monoxide, oxygen, and heavy hydrocarbons. Only *B. nacraceus* and *B. pseudosubtilis* produced these latter gases. The conditions under which these mixtures of combustible gases were produced were perfectly comparable with those that would occur in nature, and there is therefore every reason for assuming the possibility of their production by bacteria in coal measures.

Hence, while the spontaneous ignition of coal cannot be attributed exclusively to bacteriological activity, it is not improbable that bacteria may be an important factor in its occurrence.

FIRE-PROOF AND SUBMARINE PAINTS.—An abstract of a paper read before the Seventh International Congress of Applied Chemistry of 1909, by M. Coffignier, is published in the *Journ. Soc. Chem. Ind.* (1911, xxx., 223). In fire-proof paints the principle adopted is to incorporate with the other ingredients of the paint an ammonium salt, which under the influence of heat, will give off ammonia, and so produce an atmosphere unfavourable for combustion. The solubility of most ammonium salts renders them unsuitable for this purpose, but good results have been obtained by mixing the pigment with insoluble ammonium magnesium phosphate and a special medium consisting of linoleate of lead in oil of turpentine.

In 1895, a special submarine paint was prepared by Holzapfel, the object being first coated with an anti-corrosive deposit, and then with a second layer containing toxic substances. Owing to the reactions which occurred between the two layers, however, the paints were liable to crack, and would not last for more than about six months.

Recently this drawback has been remedied by the production of a paint in which the outer coating consists of an amalgam of copper incorporated with an earthy pigment and a waterproof medium.

As soon as marine organisms attack this coat the amalgam is exposed, and voltaic currents are produced which set free poisonous compounds of copper and mercury and destroy the intruders. Thus the action is only brought about in places where it is necessary, and the life of the paint is doubled.

GEOLOGY.

By RUSSELL F. GWINNELL, B.Sc., A.R.C.S., F.G.S.

THE GEOLOGICAL SURVEY OF SCOTLAND.—Three colour-printed sheets of the one-inch map of Scotland have recently been issued, together with memoirs on the same districts. Two of these (Edinburgh and Haddington) are new editions; the third is the long-expected Glenelg sheet, which includes

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the eastern half of the Isle of Skye. This Glenelg sheet, in particular, is a splendid example of colour-printing. Nearly sixty different shades of colour (including hatched and stippled patterns) are used, and a large number of dykes and sills are mapped, so that altogether the map is very complicated; but nevertheless the use of colour-printing has made it possible to issue the sheet at the low price of half-a-crown. The issue of this sheet and memoir is opportune, as the Geologists' Association (of London) will be visiting the district this coming summer, under the guidance of one of the Survey officers, Mr. Alfred Harker, M.A., F.R.S., whose work on the Tertiary Igneous Rocks of Skye is so justly celebrated.

The ancient Lewisian and Moine rocks of the Glenelg area are dealt with in detail in the memoir and some interesting lithological types described, such as the brilliantly-coloured eclogites and garnet-amphibolites, with a garnet-fuchsite rock (containing the vivid green chrome-mica, fuchsite). Lewisian limestones occur containing various silicates, especially diopside, which sometimes forms masses up to several yards across, also abundant forsterite crystals and great poikilitic plates of phlogopite enclosing grains of calcite. As to the puzzling Moine schists some evidence appears to indicate that these were laid down *unconformably* on the Lewisian gneiss, and, while no definite conclusion is reached, various facts suggest that they may be altered representatives of Torridonian sediments.

A large relief-model of Central Skye has been constructed, which brings in most of the western half of this Glenelg sheet. There are copies of this model at the Survey Museum (Jermyn Street), at the Science Museum (South Kensington), at the coming Coronation Exhibition, and elsewhere.

The memoir on "The Geology of the Neighbourhood of Edinburgh" was first published in 1861. The new edition, the second, forms a valuable guide to this interesting district. The main geological interest lies, perhaps, in the igneous rocks, and the petrography of these forms an important part of the volume. Noteworthy are the numerous highly-alkaline types, often containing analcite, as, for example, the associated Essexites, Teschenites and Picrites, as well as Mugearite. This last peculiar type, first described by Harker from the Tertiary Series in Skye, and recorded in this memoir from the Carboniferous of Edinburgh, has also been found in the neighbourhood of Glasgow, while within the last few weeks Mr. H. H. Thomas has proved its occurrence in the Ordovician volcanic series of Skomer Island, Pembrokeshire (*Abstract of the Proceedings of the Geological Society*, February 1st, 1911). The main peculiarity of this type lies in the paragenesis of olivine with such alkaline felspars as oligoclase and even orthoclase.

The structure and age of the old volcano of Arthur's Seat, Edinburgh, has long been a subject of contention. On the one hand MacLaren, over seventy years ago, maintained that it represented two entirely distinct series of volcanic outbursts, separated by a vast interval of time, the older being of Lower Carboniferous age. This interval was marked by the deposition, subsequent upheaval and removal by denudation of at least three thousand feet of Carboniferous strata. In the first edition of the present memoir, Geikie followed this view and considered the supposed younger volcanic series to be probably of Tertiary age, but later he referred them to the Mesozoic, and then to the Permian. Professor Judd, in 1875, advocated the theory that the supposed second series of volcanic outbursts had no existence, but that all the rocks are the result of a single and almost continuous series of eruptions confined to the Lower Carboniferous period. The evidence obtained during the recent revision of Arthur's Seat by the Geological Survey has confirmed Professor Judd's contention.

ORIGINAL GNEISSOSE BANDING.—The term "gneiss" covers a multitude of rocks, which differ vastly not only in composition, but in mode of origin. Their only community lies in the possession of that foliated character known as the gneissose structure. Some have been derived from sedimentary rocks (the *paragneiss* of Rosenbusch, *epigneiss* of Reusch and *metagneiss* of Lepsius); others are of igneous origin (*orthogneiss* of Rosenbusch), while in others (the *protogneiss* of Lepsius) some see the primary crust of the earth. Yet another type, the *adergneiss* of Sederholm, is both igneous and sedimentary in origin, the foliation being the result of the injection of many veins of pegmatite into a sedimentary rock, so that the sediment becomes thoroughly permeated by igneous material. This type is described among the Lewisian rocks of the Glenelg area in the memoir already referred to in these columns.

But confining our attention to orthogneisses only, we find several distinct types, of which the most usual has—at any rate in the past—been regarded as due to regional metamorphism. Professor G. H. Williams was one of the early exponents of the school which laid great stress on this mode of origin. Another possible means of producing foliation in igneous rocks (of plutonic type) is by successive intrusions of magma of different composition into the same consolidation site. Thus Harker accounts for the coarser banding of the peridotites of Rum in the Inner Hebrides. There remain two very similar methods, the intrusion of one molten mass already heterogeneous and the simultaneous intrusion of two different magmas. The finer banding of the Rum peridotites and the foliation of gabbro in the Cuillin Hills, Skye, have been explained in this manner, as well as a perfect banding in the dioritic complex of the Island of Ornö, near Stockholm.

The foliation in the Cortlandt Series—an igneous complex ranging from granite through syenite, monzonite, diorite, gabbro and norite to pyroxenite and peridotite, occurring about thirty-five miles north of New York City—has been dealt with by several authorities. Professor J. D. Dana regarded the rocks as worked-over sediments, volcanic ashes or tuffs which, on being subjected to intense local metamorphism, lost most of their bedded structure and became pseudo-massive; later he treated them no longer as paragneisses but as of igneous origin (orthogneisses), and Dr. G. H. Williams, in 1886, regarded the foliation as due to regional metamorphism. In *The American Journal of Science* for February, 1911, G. S. Rogers demonstrates the origin of these rocks (where norite and pyroxenite form alternate layers of constant grain) to be by "magmatic differentiation," as in the cases of the Rum peridotites and the Skye gabbros.

Up to the present this original gneissoid banding has not been recognized in many localities; it may, however, prove illuminating (especially if found to be more common than is at present thought) in connection with some of the puzzling structures of the ancient and obscure igneous gneisses.

THE HARDNESS OF MINERALS.—In spite of all criticisms Mohs' scale of hardness still holds its place as the standard of reference in Mineralogy. Breithaupt interpolated two extra minerals between numbers 2 and 3 and 5 and 6 respectively, converting Mohs' scale of ten minerals into a scale of twelve, in the attempt to make the intervals between successive numbers more uniform. By various methods of estimating "absolute hardness," Pfaff, Jaggar and others, shewed that the intervals are far from uniform, and thus the series of figures representing absolute hardness form a progression which does not approximate either to arithmetical or geometrical progression. But most noteworthy are Rosiwal's figures, obtained in 1892, which show that Topaz (number 8 in Mohs' scale, where 1, the softest, is Talc, and 10, the hardest, is Diamond) is softer than Quartz (number 7). Thus in detail *inversion* of the correct order is indicated.

Rosiwal obtained his figures by using a standard abrasive to grind the mineral surface, and determining the loss of weight suffered by the mineral when a given weight of the abrasive was used up. In *The American Journal of Science*, February, 1911, H. Z. Kip describes how he obtained a similar result by a different method, which depends on the force required to produce abrasion on the mineral, by a diamond-point sclerometer.

"Pfaff, Jaggar and others who arrive at the opposite conclusion, have failed to eliminate the factor of density in carrying out their tests. In other words, while regarding hardness as *resistance to abrasion*, they have sought to determine its value on the theory that it was to be measured in terms of *resistance to excavation*."

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METEOROLOGY.

By JOHN A. CURTIS, F.R.MET.SOC.

THE WEATHER of the week ending February 18th as detailed in the *Weekly Weather Report* issued by the meteorological office, was very warm for the time of year, with heavy rain in Scotland and Ireland and the Northern Counties of England, and moderate to scanty sunshine. The excess of temperature varied from 5°·6 in Scotland E., to 0°·6 in the English Channel. The maxima were above 50° at nearly every station, and reached 59° at Aberdeen on the 16th, and at Bawtry on the 18th. At Guernsey, however, the highest reading was only 51°. The nights were cold, and the minimum fell to 18° at Balmoral on the 12th, and to 21° at Cirencester and Swarraton on the same day. On the grass the temperature fell to 10° at Llangammarch Wells, and to 12° at Newton Rigg.

Rainfall was frequent and heavy in the North. At Glencarron it rained every day, and the total for the week was 6·94 inches; at Fort William 5·39 inches were collected. At Westminster the total was only 0·25 inch in three days.

Sunshine varied a good deal in different parts. At Newquay the total duration was only 7·0 hours, as compared with an average of 19·4 hours, while at Eastbourne it was 29·7 hours, or 11·3 hours above the average.

The mean temperature of the sea water varied from 47°·5 at Scilly to 38°·1 at Cromarty.

The weather of the week ended February 25th was very changeable. There was a good deal of rain, but bright intervals were common. Snow was plentiful in parts of Scotland, where also thunderstorms were experienced. Aurora was seen on several nights during the week.

Temperature was still unusually high in all parts. The highest reading was 59° at Dublin on the 21st, but readings of 58° were reported at several stations from Leith to Fulbeck (Lincoln) and Birr Castle. The lowest readings reported were 21° at Sunburgh Head and Balmoral. At Guernsey, however, while the maximum did not exceed 52°, the minimum did not fall below 40°, a range of 12°, as compared with a range of 28° at Sunburgh Head (49° to 21°). The lowest readings on the grass were 15° at Llangammarch Wells and 16° at Crathes.

Rainfall was in excess in all Districts except the English Channel, where it was just below the normal. In some places very heavy amounts were collected—thus at Glencarron the total was 5·01 inches, and at Fort William 5·60 inches. Including the amounts collected in the previous week the totals at these two stations for the fortnight were 11·95 inches and 10·99 inches respectively, or 8·39 inches and 7·45 inches, above the averages for the same period. In spite of the generally heavy rain sunshine was abundant in all districts. Dublin was the sunniest station with 36·9 hours (53%) but many stations reported upwards of 45%, Ventnor 47% and Torquay 48%. In Westminster the total duration was 24·5 hours or 35%. The temperature of the sea water varied from 49°—47° at Scilly to 39°—34° at Cromarty.

The week ended March 4th proved to be unsettled, with frequent rains. Aurora was seen in Scotland on the night of February 28th. Temperature continued above the average in all districts, the excess amounting to as much as 6°·7 in the Midlands, where 60° was reported as the maximum at Raunds, on March 2nd; 60° was also reported at Westminster on the same day. The maximum for the week at Jersey was 53° on the 3rd, on which day Strathpeffer reported 54°. The lowest minimum was 22° at Balmoral on the 27th. In Ireland and in the South of England no frost was experienced during the week. On the grass minima down to 19° were observed.

Rainfall was also in excess, except in England N.E. In England N.W. and S.W. the total for the week was double the average, but individual heavy falls were rare. Sunshine varied in different districts. In Scotland E. it was 11 hours (16%) in excess of the average, while in the Channel Islands it was 5 hours (6%) in defect. Crathes reported the most sunshine, 36·7 hours or 51%. At Westminster the duration was 18·3 hours, 25%. The temperature of the sea water varied from 51° at Seafield to 33° at Wick.

The week ended March 11th, was cooler and drier than those that had preceded it. Temperature was, however, still above the average in many places, though not to any great extent. In the South it was slightly in defect. The maximum for the week was 57° at Killarney, on the 8th, but at no other station in the British Isles was a reading higher than 52° reported. In many places the maximum was below 50°. Frost was experienced in all Districts except the English Channel, the lowest readings being 24° at Balmoral, and 25° at Fort Augustus and Llangammarch Wells. On the grass the temperature fell to 13° at Llangammarch, and to 20° at Kew and Tunbridge Wells.

Rainfall was variable. It was heavy in Scotland N. and in the South of England and Ireland, but not far from the normal in other parts, generally slightly below.

Sunshine as a rule was above the average, and the District percentages ranged from 46% in Scotland E. to 24% in the Midland Counties. The sunniest station was Gordon Castle, 40·5 hours or 53%. Westminster reported 10·4 hours or 13%.

The mean sea temperature was 47°·4 at Scilly and 38°·5 at Cromarty.

UPPER AIR RESULTS.—On February 14th, a kite at Pyrton Hill, when three thousand six hundred feet above the ground, entered a current of air, which, though moving in nearly the same direction, was 9° F. warmer and much drier, and had a velocity of 50% greater than that immediately beneath it.

On the 16th, at Brighton, a kite sent up by Mr. S. H. R. Salmon entered the clouds at only three hundred feet above the ground, and became unmanageable owing to the great increase in wind velocity.

ANCIENT RAINGAUGES.—It has generally been accepted that the first raingauge of which we have record was made by an Italian, Benedetto Castelli, a contemporary of Galileo, in 1639, but Dr. Y. Wada, the Director of the Korean Meteorological Observatory at Chemulpo, has given in the "Scientific Memoirs of the Korean Meteorological Observatory," Vol. I., an interesting account of the installation of a number of raingauges and the organization of a system of rainfall observation in the year A.D. 1442, or one hundred and ninety-seven years before Castelli. The account, which has been unearthed from the Korean historical records, tells how "King Sejo caused an instrument of bronze to be constructed to measure the rain. This is a vase fifteen inches deep and seven inches in diameter, placed on a pillar. The instrument has been placed at the Observatory, and each time rain falls the Officials of the Observatory measure the height with a measure and make it known to the King. These instruments were distributed to the Provinces and Cantons, and the results of the observations were sent to the Court."

MICROSCOPY.

By A. W. SHEPPARD, F.R.M.S.,

with the assistance of the following microscopists:—

ARTHUR C. BANFIELD.
JAMES BURTON.
THE REV. E. W. BOWELL, M.A.
CHARLES H. CAFFYN.
ARTHUR EARLAND, F.R.M.S.
RICHARD T. LEWIS, F.R.M.S.
CHAS. F. ROUSSELET, F.R.M.S.
D. J. SCOURFIELD, F.Z.S., F.R.M.S.
C. D. SOAR, F.R.M.S.

TRANSMISSION OF FLAGELLATES IN FRESH-WATER FISHES.—At the meeting of the Royal Society held on February 23rd, Miss M. Robinson, M.A., read a paper on the above subject, of which the following is an abstract.

The goldfish in a pond at Elstree have for some years shown an infection of trypanosomes in their blood. Quite recently trypanoplasma has also appeared. Upon investigation it was found that the leech *Hemiclepsis marginata* occurred in the pond and effected the transmission of the parasites.

A large number of these leeches were obtained from the Grand Junction Canal reservoir, which is only a short distance from the pond. The young of these were hatched out in captivity, and it was ascertained that the flagellates are not

passed from parent to offspring. The parent leeches were invariably infected with trypanosomes derived from the fish in the reservoir, which frequently showed these parasites in their blood.

The trypanosomes of perch, bream and goldfish were found to complete their cycle in *Hemiclepsis*, and could be transmitted to clean goldfish by means of leeches. The specimens used in these experiments were always young laboratory-hatched *Hemiclepsis*. The trypanosomes of pike and rudd also complete their cycle in this leech, but the opportunity of passing these two forms into goldfish did not present itself. The cycles of the trypanosomes derived from these different sources are apparently identical. The main features are as follows:—

The trypanosomes taken into the crop of the leech along with the blood multiply very rapidly, undergoing a marked change of form. After some days slender forms begin to arise. These increase in number, and at the end of digestion, some time after the blood has quite disappeared, they come forward and lie in the proboscis-sheath in very large numbers. The form found in the sheath is a very slender, long creature of quite definite type; division has never been observed in this phase.

When the leech feeds once more, these individuals are inoculated into the fish. The proboscis-sheath is always cleared of trypanosomes by one feed. After a clean feed the slender inoculative type of trypanosome disappears from the crop of an infected leech, and the infection is carried on by short, broad forms. Conjugation has never been observed.

If water is added to the blood of fish containing trypanosomes, the flagellates divide after a number of hours, probably in response to lowering of osmotic pressure in the fluid in which they find themselves.

ROYAL MICROSCOPICAL SOCIETY.—February 13th, 1911, Mr. H. G. Plimmer, F.R.S., President, in the chair. —Messrs. E. Heron-Allen and Arthur Earland read a paper illustrated by a series of lantern slides on new or rare species of Foraminifera found in the shore-sands of Selsey Bill, Sussex. The authors called attention to the identity of the fossil Foraminifera of the Bracklesham Beds with the living species found in Australian shore-sands. Recent specimens of *Bolivina durrandii* Millett and *Pulvinulina vermiculata* Brady, were shewn, the only other known records being as regards the former from the Malay Archipelago, and as regards the latter from tropical and sub-tropical seas. In addition to these, *Miliolina suborbicularis*, *M. rotunda*, *Textuluria inconspicua*, *var jugosa*, *Bolivina tortuosa*, *Uvigerina asperula*, *and Sagraina dimorpha*, were recorded as new to Britain. Schlumberayer's unique genus and species, *Hinderina brugesii*, was recorded from the Eocene clays. Also the first fossil records of *Bulimina subteres* and *Discorbina polystomelloides*. The new species recorded were *Pulvinulina haliotidea* H. A. and E., and *Nonionina quadriloculata*, H.A. and E. Specimens of these were exhibited under microscopes during the meeting.

Mr. Lees Curties described a new dark-ground illuminator which he had made to the instructions of Mr. E. M. Nelson, and which was so constructed as to work with slips ranging from 0·8 to 1·2 millimetres in thickness, and which gave a perfectly dark field with a Zeiss apochromatic four millimetres lens of 0·95 N.A. The illuminator was provided with a fixed central stop, and also with a slot for utilising the apparatus as an oblique illuminator. A small dot placed on the front lens served for the purpose of centring the condenser to the optical axis.

SOME WORKS REFERRING TO RED-SNOW.—Mr. James Murray has kindly prepared the following short bibliography on the subject of his paper (see "KNOWLEDGE," page 109); it will be of use to those readers who are desirous of carrying further the subject of "Red-Snow" and its occurrence.

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QUEKETT MICROSCOPICAL CLUB.—February 28th, 1911.—Mr. D. J. Scourfield, F.Z.S., F.R.M.S., Vice-President, having taken the chair, the President delivered his annual address dealing this year with "Some Problems of Evolution in the Simplest Forms of Life."

Taking, first, the visible world of living creatures, no deep reflection or analysis is required to grasp the fact that it does not constitute a chaos of isolated and unconnected forms, but is capable of being classified into greater or lesser categories. The first and most obvious division is into animals and plants.

But between the categories of greatest and least extent there are a number of intervening divisions, with regard to which the scientific and the non-scientific public are hopelessly at variance. The same is inevitably true in any branch of knowledge dealing with a variety of concrete objects, simply because the mind devoted to the study of any particular set of things, animate or inanimate, soon becomes perforce acquainted with so many more than ever come within the ken of the casual observer, that in order to arrange them in an orderly and intelligible system of classification, it is necessary to draw distinctions and institute comparisons which are never dreamt of in the philosophy of the mind occupied with other pursuits.

Professor Minchin then proceeded to review various systems of classification popularly recognised, as, for instance, the separation of vertebrates into two main groups, one with paired limbs, as in fishes, and the other in which the paired limbs are pentadactyle in type. Then, again, animals may be divided by their habitat into terrestrial, aquatic and aërial. But to all these systems we at once find exceptions. Many perfectly logical classifications are possible, but only one that is perfectly natural, and that one, very often, is not perfectly logical. It is now abundantly clear that natural groups can seldom, if ever, be defined by precise and rigorous verbal definitions. All that can be done is to construct for each group a more or less ideal and imaginary type of organism, possessing certain characters, none of which must be regarded as fixed or invariable. If we must have verbal definitions of groups, then logic requires the insertion of the word "typically" before each character ascribed to them.

In dealing with the Protista, the President considered that there are two well-marked types recognisable in these organisms, one more primitive and older in evolution, the other higher, and leading on to the ordinary plants and animals. The difference between these two types depends on the condition under which that peculiar substance occurs for which we may use, in quite a general sense, the term chromatin. In every cell of animal or plant, and probably in every Protist

organism, there is found a certain amount of a substance remarkable for its affinity for certain colouring matters, and still more remarkable for the part it plays in all vital processes. From the first of these characteristics the name chromatin has been given to it. It is a substance, or combination of substances, of a very high degree of chemical complexity, perhaps more complex than any other substance, but it is by no means of uniform chemical composition. The term chromatin implies, in short, a biological or physiological, but not a chemical, unity.

In the lower type of Protist organisation, which is exemplified by the ordinary Bacteria, the chromatin is present in the form of scattered granules—"chromidia." In the second type a certain amount of the chromatin may still be present in the scattered chromidial condition; but the greater part, and in most cases all of the chromatin, is aggregated into a compact mass, the nucleus, and, apart from this nucleus, the remainder of the living body is made up of a distinct protoplasmic zone—the cytoplasm, scarcely recognisable in the bacterial type. With differentiation of nucleus and cytoplasm, the organism becomes a "cell." This type will be termed the "cellular grade." Reference was then made to the existence in all forms of higher life of sex and sexual differentiation, and even in the Protista we find sexual phenomena to be of universal occurrence in the cellular grade, but quite absent in the organisms of the bacterial grade. Much has been written, and many theories put forward, to explain the origin and significance of sex. Professor Minchin dealt briefly with only one, that put forward by Doflein, and founded by him on those enunciated previously by Hertwig and Schaudinn. This theory is, shortly, as follows:—Living cells are regarded as consisting of two groups of vitally active substances, the one regulating motor, the other trophic, functions. In cell-reproduction by fission, these substances are never distributed with mathematical equality amongst the descendants; hence continued reproduction of this kind brings about accumulations of different properties in certain individuals, with, as a consequence, impaired vital activity and reproductive power. Individuals are produced, some of which are richer in stored-up nutriment (female), others in motile substance (male). Since these two kinds of individuals contain aggregations of substances which have intense mutual chemical reactions, they exert an attraction one towards the other; the two individuals tend to unite, and by their union cell-equilibrium is restored and vital powers renewed. Hence syngamy is regarded as a necessity for the life-cycle, due primarily to the imperfections of cell-division and to the consequent loss of equilibrium in the cell-constituents. On this view, the general absence of sex phenomena in the lowest grade, and its existence in the higher, is readily intelligible. In the bacterial grade, the body, usually very minute, is of extremely simple structure. In such organisms, inequalities of cell-division, if they occur, can be adjusted easily by the rearrangement of the chromatin substance. On the other hand, with the evolution of the cellular grade, the body is differentiated into at least two parts, nucleus and cytoplasm, and becomes of increasingly complex structure. Consequently, an exact quantitative and qualitative partition of the body during cell-division is of extremely improbable occurrence—at least, until the mechanism of cell-division has reached its greatest perfection. The fact that in the Infusoria, the most complex in structure of all the Protista, syngamy is a frequent event and easy to observe, fits in also with the view that sex phenomena are in relation to complication of cell structure; and, conversely, the fact that in Protozoa of simple structure, such as the Flagellata, syngamy is rarer, and appears only to occur at long intervals in the life-cycle, also receives a simple explanation. From all these facts and considerations, it appears extremely probable that sex and syngamy were "invented" when the cellular grade was evolved from the bacterial grade of structure. This, again, the speaker said, is related to another very important property of living things—the more or less easy divisibility into groups which we now term species. No one now considers a species as a fixed and immutable entity. Nevertheless, the fact remains that the tendency of living things to separate themselves into species more or less distinct is one of the most constant and universal pecularities of the organic world.

From these considerations it is evident that the passage from the bacterial to the cellular grade was, perhaps, the most important advance in the evolution of living beings. The acquisition of the cellular type of structure was the starting-point for the evolution, not only of the higher groups of the Protista, but, through them, of the whole visible everyday world of animals and plants, in all of which the cell is the unit of structure, and which consists primarily of aggregates of cells. Further, with the cellular type of structure were initiated, in the speaker's opinion, two of the most universal and characteristic peculiarities of living beings—namely, the phenomena of sex and the tendency to form species.

FIGURE 1.

AN INTERESTING MICRO-OBJECT FROM A SINGULAR SOURCE.—The fungus known popularly as the truffle, and used as a flavouring, affords some interesting and instructive microscopical objects. At the same time it is not exactly easy to obtain, and it occurred to me to resort to the "truffle and liver sausage," so much in evidence at the foreign "*delikatessen*" shops, frequently found in some parts of London. The result was perfectly satisfactory; a thin slice of the delicacy yielding ample material for examination, and for mounting hundreds of slides had such been desired.

Several different species of the family *Tuberaceae* do duty as "truffles" in the popular sense, nor do they differ much from one another in character. As their name implies, they bear an outward resemblance to a tuber, such as a small potato or artichoke. Inside they are composed of *hyphae*—the much-branched and anastomosing tubular threads—which make up the greater part of all fungi. These are packed closely together, but numerous spaces and channels are found in which on the ends of the hyphae are developed the asci, or spore sacs. In most members of this family these are more or less spherical and contain typically four spores. In my specimen the bundles of hyphae are of a grayish tint, while the intervals with the numerous sacs containing brown, almost black, spores show as a darker mottling on the ground mass as represented in Figure 1a, which is a portion only slightly magnified (about × 5). There may be from one to four spores in an ascus and they are not formed simultaneously (Goebel), so that under the microscope, while one may be fully developed, others show earlier stages. The outer coat of the mature spore is thick and strongly cuticularized and is ornamented throughout the family with projections of various kinds. In the present species the large and handsome spores are covered with sharp spikes and points, Figure 1b. A second thinner coat within is easily made out with careful focussing. The young spores have less colour, and according to age the characteristic markings of the outer coat are not so fully developed. As in most of the very large class of ascomycetous fungi, the asci are elongated tubular sacs, while the typical number of spores in them is eight,—though exceptions are not few—the family brought under notice forms an interesting variety for comparison; while also many of its life processes, such as to some extent its method of growth from the spores and details of the formation of its fructifications, are still unknown. J. B.

ORNITHOLOGY.

By HUGH BOYD WATT, M.B.O.U.

THE BRITISH ORNITHOLOGISTS' UNION EXPEDITION TO CENTRAL NEW GUINEA.—The two members of this party invalided home,—Messrs. W. Goodfellow and G. C. Shortridge—have recently safely reached London, and the last-named has brought with him a large zoölogical collection, including about eleven hundred birds. One Paradise-bird is stated to be certainly new to science, and there are fine examples in the collection of the scarlet and yellow Paradise-bird (*Xanthomelas ardens*), new to the National collection at South Kensington; the female of the species was hitherto unknown. King-birds, rifle-birds, and manucodes are well represented; cat-birds, grackles and starlings of several species are numerous, and parrots of all sizes, from pygmy parrots to great black cockatoos; also kingfishers, rollers and pittas. The cuckoos include a rare and curious species (*Microdynamis parva*) resembling a honey-guide. There are, further, many different species of pigeons and fruit-pigeons, and numerous smaller birds, which are expected to yield the most interesting forms of all, when they have been studied by the authorities.—(*Country Life*, 4th March, 1911, page 291.)

In Ethnology the important discovery has been made by this expedition of a community of primitive people, wearing no clothes, having still in use the implements and weapons of the Stone Age, and unable to count beyond three. These people occur up the Mimika River, in the hitherto unknown interior of Dutch New Guinea. Lantern slides from these were shown at the March meeting of the British Ornithologists' Club.

WATERFOWL IN THE ZOOLOGICAL GARDENS, LONDON.—Amongst recent improvements at the "Zoo" is a new lake for waterfowl, the stock of which is heavier now than it has been for a long period. It is hoped that one result may be a very full list of birds breeding this season.

The geese include examples of the Orinoco, upland, ruddy-headed, emperor, cereopsis, snow (blue and white), black-headed, bar-headed, Hutchins, and spur-winged. On the water may be found the common, ruddy and Argentine flamingoes, mandarin, summer and Bahama ducks, spoonbills, American and Chiloe widgeon, China pintail, cinnamon, blue-wing, green-wing, Japanese, chestnut-breasted, Andaman, Brazilian, Chilian, and versicolour teal, rosy-bills, red-crested pochards, white-eyes and maned geese. In the sea-lion's pond is a recently arrived king penguin, about three feet in height.—(*The Field*, 25th February, 1911, page 384.)

CROSSBILLS IN THE BRITISH ISLES.—An account of the exceptionally wide-spread visitation occurring in 1909–10, and the phenomenal nesting which followed, is promised by Mr. H. F. Witherby, of *British Birds*, and the subject was illustrated by exhibits made at a recent meeting of the British Ornithologists' Club. Nests and eggs were shown, most of the eggs being those of the Continental crossbill, obtained last season in different parts of England and Ireland. Eggs were also exhibited from the Continent, as well as those of the resident Scottish race of this bird and of the parrot crossbill, for comparison. The nests showed much divergence in make and material. The Rev. F. C. R. Jourdain pointed out that the material for study had hitherto been scanty, and that there was no authentic record of a case of nesting in the same locality in England for two consecutive seasons.

THE WOOD-PIGEON PEST.—The number of resident birds of this species in Wilts, Hants, and other Southern Counties this winter has been enormously swollen by arrivals from other quarters. Many of these are smaller in size, and recognisable as immigrants from Scandinavia. The birds were abnormally prevalent and persistent, doing havoc to clovers and catch-crops, which are specially cultivated in the counties named for ewes and lambs in the spring months. A flock-master at Cholderton, near Andover, with four thousand acres of ground, estimated that some six thousand to seven thousand pigeons frequented it. In Wiltshire alone the damage done by the middle of February was said to mount up to about £30,000. Ordinary methods of scaring and trapping having proved quite insufficient, a vigorous shooting campaign was organised. One, two, or three days in each week have been given to shooting the birds in each neighbourhood simultaneously, as far as possible, following them to their roosting-places. A Government Inquiry is being asked to investigate the extraordinary increase in the species, and how to deal with this plague to agriculturists.

A GOOD LOCAL LIST.—"The Birds of East Renfrewshire," by Mr. John Robertson (*Glasgow Naturalist*, February, 1911, pages 41–59; 207, Bath Street, Glasgow, 1/3), is one of those somewhat notable little works which show how rich a result follows continuous and careful observation, even in a not too favourable locality. The district covered is a limited inland one, encroached upon further and further each year, by the southern suburbs of Glasgow. Yet Mr. Robertson records one hundred and fifty-one species, eighty-three of which have bred. The number of kinds of ducks and waders which occur, season after season, is remarkable.

THE LATE PROFESSOR ALFRED NEWTON.—Those who only knew this great English ornithologist by his own writings and the eulogistic accounts which have been given of him in scientific and ornithological publications will turn, with great interest to an article in the March number of the *Cornhill* (pages 334–349), by Mr. Arthur C. Benson, who was a contemporary of the Professor at Magdalene, Cambridge, from 1904 to 1907. Mr. Benson's chapter is, of course, not ornithology, but is an intimate and personal narrative, revealing a character of a type probably not anticipated by those who had not personal acquaintance with the man. From this personal point of view, Mr. Benson's own feelings are thus expressed:—"I began by fearing him, I went on to admire him, and I ended by loving him."

"PHOTOGRAPHY FOR BIRD-LOVERS."—Messrs. Witherby & Co. have in the press an illustrated volume entitled "Photography for Bird-Lovers," by Mr. Bentley Beetham, a well-known and successful bird photographer. The book is an essentially practical guide to the pursuit of bird-photography in all its branches, and Mr. Beetham gives ungrudgingly from the store of knowledge gained by his own personal experience in the field.

PHOTOGRAPHY.

By C. E. KENNETH MEES, D.Sc., F.C.S., F.R.P.S.

THE RELATION BETWEEN THE COLOUR OF SILVER IMAGES, AND THE SIZE OF THE PARTICLES WHICH COMPOSE THEM.—In a former number of "KNOWLEDGE" (December, 1910), I referred to the investigation of this subject by Schaum and Schloemann, in which they came to the conclusion that the colours were due to optical resonance.

At the Royal Photographic Society, on March 7th, Mr. Chapman Jones gave an account of an investigation undertaken by him with a view to the examination of the accuracy of Zsigmondy's view that it was not possible to deduce the size of the particles from the colour of a colloidal solution, and that the colour was dependent to a considerable extent upon the separation between the particles.

As the subject of this investigation, bromide of silver, chloride of silver and phosphate of silver lantern plates were used, developed by means of developers containing ammonium carbonate and ammonium bromide, so as to produce carmine and yellow tones; while in one series of experiments the phosphate plates were printed out.

Mr. Chapman Jones adopted a most ingenious method of measuring the size of the particles. If a silver image be treated with mercuric chloride and, after washing, developed with ferrous oxalate, it adds mercury in the proportion of one atom of mercury to one atom of silver. A second intensification produces an image consisting of one atom of silver and three atoms of mercury; a third, one atom of silver and seven

atoms of mercury; so that the particles grow rapidly in size, and from the known specific gravities of the various amalgams the increase in the diameter of the particles can be calculated. Eight such intensifications produce an enlargement of over seven diameters, and even particles which were at first completely ultra-microscopic may be thus rendered measureable.

The method appears to be capable of giving results of considerable accuracy, probably of the order of five per cent, and the results obtained were satisfactory concordant. They showed that in films, where the particles were too small to produce any visible colour, only a faint blue opalescence being visible, the diameter of the particles ranged from ·10 to ·12 microns. If the films were yellow the diameter was ·13 to ·14 microns; ·15 to ·17 gave orange or brown films, and in one case pink, while ·17 to ·18 gave purple tones, diameters above ·18 corresponding to brown-blacks and greys.

These diameters are somewhat less than those found by Schaum and Schloemann for their grains.

The refractive index of gelatine was measured by a number of methods, the most successful being the finding of a liquid mixture of known refractive index in which the edge of the film disappeared; the result obtained was 1·53. Now it will be seen that the diameters given for the various particles nearly correspond to the half wave-lengths in gelatine of the light whose absorption would produce the colour stated, thus the wave-length of the limit of the visible spectrum, ·4 microns in air, will become ·26 microns in gelatine, the half of which, ·13, corresponds well with the diameter of the particles which commenced to show a yellow colour.

Mr. Chapman Jones gave some remarkable instances of the accuracy of the relation found between the colour of the deposit and the size of the particles; he had examined images of mixed colours and had always found that such images contained particles of at least two different diameters, corresponding with the different colours.

He had also found that where the particles were of the same size, but were dispersed to different extents, the colours were the same, thus showing that Zsigmondy's suggestion that the colour depends on the dispersion of the particles cannot be substantiated for silver particles in gelatine films.

These results obtained by Mr. Chapman Jones are entirely different from those of Schaum and Schloemann, who experimented with a chloride emulsion developed with various developers, and also with gelatine films containing silver nitrate printed out, and who investigated the subject by means of the ultra-microscope.

The conclusions to which these workers came were :—

(1) In coloured images obtained under their conditions the ground was coloured and the particles of considerable size, and black, in opposition to the results of Kirchner and Zsigmondy, who found that in gold gelatine preparations the ground was colourless and the particles strongly coloured.

(2) The colour was unaltered by diluting a coloured film with gelatine and re-coating and drying. The colour was also little altered by intensification, bright red becoming dark red, bright green, dark green, yellow passing through orange to red, and blue becoming deep violet. The colour was also little altered by reduction, simply becoming less saturated.

(3) The colour is associated with the thickness of the layer of grains, as was found by Kirchner for Lippmann films, rather than with the size of the particles.

It is to be hoped that Mr. Chapman Jones' valuable paper will lead to further work upon the subject, which may result in a reconciliation of these apparently conflicting results.

THE EFFECT OF COLOUR FILTERS UPON THE DEFINITION OF A LENS.—If colour filters be used with a lens, it is clear that considerable attention should be paid to the optical accuracy of those filters, so that they do not introduce aberrations which may affect the definition of the image. Apart from the accuracy of the glass itself, distortion may be produced in colour filters in the course of their manufacture in several ways.

In the first place, if the filters are prepared by coating coloured gelatine upon the glass, then when this gelatine dries it will contract and bend the glass; also, when the filter is cemented with Canada Balsam, too rapid drying or drying at uneven temperatures will distort the filter; while, finally, if pressure is exercised upon a thin filter in its cell, the filter may easily be permanently strained. If these strains were symmetrical they would be of small importance, as they would simply produce a lens of slight positive or negative power, and so, to a small extent, change the focal length of the lens with which they are used. But generally they are either in one direction only, or are much greater in one direction than in the other, and so produce a cylindrical lens, which introduces astigmatism. The effect of such aberration naturally becomes much greater as lenses of longer focal length are used, the effect varying as the square of the focal length of the lens, so that a filter which would be perfectly satisfactory on a hand camera lens of six inches focus, would be with a telephoto combination quite useless. With medium and high power telephoto lenses only filters of the highest optical accuracy can be used.

This point must be carefully borne in mind, in view of the recent introduction of what may be termed semi-telephoto lenses, such as the Busch Bis-Telar, which naturally require that a filter should be far more accurate than would be assumed to be necessary for its diameter.

The aberrations of filters can be minimised by making them of as thick glass as possible, having regard to its optical accuracy, and for filters of the very highest quality it is usual for the two glasses to be about five millimetres in thickness.

PHYSICS.

By A. C. G. EGERTON, B.Sc.

GENERAL.—Among the events of the last month, mention should be made that the electromotive force of the Weston Normal Cell, made up according to specification, has been accurately found to be 1·0183 international volts at 20°C.; standard cells will in future be compared with such Weston Normal Cells at the National Physical Laboratory. The Weston cell consists of an H-shaped vessel, the one limb of which contains mercury and mercurous sulphate, the other containing cadmium and cadmium sulphate. Its electromotive force is very nearly independent of temperature over the usual range of working temperatures. The E.M.F. of the Clark Cell varies with temperature, while its E.M.F. is greater than the Weston cell, the former having a zinc negative pole instead of cadmium.

Professor Perrin gave a most lucid exposition of his investigations on Brownian Movement at the Royal Institution on February 24th. During his lecture he showed, by means of cinematograph photographs, the movement that colloid particles undergo when an emulsion of gum and water is prepared and examined under the microscope: their rotation, their collision, their random path, and their attraction and repulsion from electrified surfaces were beautifully illustrated. By an able investigation of the motion of these minute colloid particles and assuming the motion of molecules to be of the same character, Professor Perrin has succeeded in calculating in several ways the number of molecules per cubic centimetre and mass of the atom of hydrogen, the numbers agreeing well with those determined by other methods, such as the period of change of radium, the charge carried by the atom of hydrogen, the polarization of light and consequent blueness of the sky, and the values obtained by measuring the viscosity of gases for the average distance traversed by the molecules between collisions or their "Mean Free Path." The number of molecules per cubic centimetre is nearly twenty-eight trillions.

THE VISCOSITY OF GASES.—In the *Proceedings of the Royal Society* there have appeared lately several interesting communications by Dr. A. O. Rankine. There is much that is pleasing about a simple apparatus: the viscosity of gases has hitherto been a somewhat troublesome co-efficient to determine, but Dr. Rankine is able to find the viscosity of Xenon, which can only be obtained in small quantity, with comparative ease, by means of his apparatus. The apparatus

XUM

consists of an elongated ring of glass tubing with a branch tube and tap sealed into the smaller sides of the ring; one of the two longer straight tubes is capillary. A mercury pellet in falling down the tube of larger bore pushes the gas in front of it through the tube of narrow bore. The time of fall is proportional to the viscosity of the gas: the more viscous the less easily can it pass along the capillary bore, and the longer the time of fall of the mercury pellet. The great advantage of the apparatus is that only small quantities of the gas under observation are needed. Dr. Rankine has observed the viscosity at different temperatures, and in this way has obtained measurements which enable him to calculate the relative attraction constants, radii and volumes of the molecules of the various gases. The gases of the argon group—helium, neon, krypton, xenon show interesting relationships respecting such constants.

THE DENSITY OF RADIUM EMANATION.—Just as in the series of compounds of carbon and hydrogen there is a group of hydro-carbons which are "saturated" and cannot combine with other atoms without replacement of those already combined in the molecule, so in the "Periodic Classification of the Elements" there is a group of elements whose atoms are unable to combine with other atoms, or which have zero valency. These elements are the rare gases above mentioned.

There is room for one or two elements more in the group according as to how the natural periodic classification of the elements is interpreted. The atomic weight of the one would be about one hundred and seventy-six, of the other about two hundred and twenty-two. Radium emanation or Niton, as Sir William Ramsay suggests should be its name, is the first break-down product of the radium atom, and is a gas behaving in every way as if it should belong to the argon group, and fit into one of these vacant places. From density determinations and observations of the boiling-point and critical point of this highly radio-active gas, it appeared that its density was such that its atomic weight should be one hundred and seventy-six. But since only one α particle or helium atom is expelled during the decay of radium into radium emanation, it was more probable that the density should be one hundred and eleven, or atomic weight, two hundred and twenty-two, unless the disintegration theory which explains the numerous transformations of radio-active substances was in fault. It was very important then to settle the point and determine the density of radium emanation.

The difficulty of such a determination would seem almost insuperable, for—to put it succinctly—it would require £15,000 worth of radium to get half a cubic millimetre of the emanation. Sir William Ramsay and Dr. R. W. Gray have, however, tackled the problem successfully. They have been able to construct a balance weighing to less than one-hundred-thousandth of a milligram, so as to be able to weigh with sufficient accuracy one-tenth of a cubic millimetre of the emanation.

It required several years of experimenting to design a balance of such sensitiveness. Dr. Brill, working under Sir William Ramsay, had improved the Nernst balance. This works by the torsion of a quartz fibre, one end of the beam acting as a counterpoise and pointer, but the deflection due to torsion of the fibre is not directly proportional to the weight. Dr. Brill's work suggested the employment of quartz knife edges and very light beams, which idea Dr. Gwyer improved and introduced a system of weighing which Dr. Steele, in Australia, independently arrived at. Sir William Ramsay and Dr. Gray were able to slightly improve Dr. Steele's balance and modify it for their purpose. A short description of the balance in a few lines is unworthy of such a beautiful instrument. It consists in the main of a fine silica rod beam resting on a specially-ground and very small quartz knife edge; from the ends of the beam is hung, by means of silica fibres, the substance to be weighed, silica counterpoises and a small silica bulb containing air. By altering the pressure inside the balance case, a small change in buoyancy of the air in the bulb is caused, so by finding the change in weight produced by a given change in pressure very fine adjustments of weight can be made; a mirror is attached to the beam of the balance, and the alteration in pressure necessary to bring back to zero the spot of light reflected from the mirror on to the scale, gives a measure of the alteration in weight.

The emanation was collected and sealed in a very fine tube which was counterpoised, then broken so as not to lose any particles of glass, the balance case evacuated so as to expel the emanation and replace it with air, then counterpoised again and the change in pressure noted.

Numerous corrections had to be made; the weight of the air entering the density bulb, after breaking it to let out the emanation; the change in the buoyancy due to the fact that the density bulb is glass and the counterpoise silica; the volume of the emanation that penetrates into the glass walls owing to its high activity, were among these corrections. The volume of the emanation was determined and the amount of its decrease before the actual determination by its change in activity.

The density came out to be two hundred and twenty-three as a mean of five experiments.

The results then confirm the disintegration theory of radioactive transformations which demands that since the atomic weight of radium is 226·5 that of the emanation should be 222·5. Further than this, they demonstrate the manipulative genius of Sir William Ramsay, and the success of these delicate experiments in the face of such huge difficulties reflects great credit on his collaborator.

MESOTHORIUM.—As an illustration of the disintegration theory, the products of change of thorium might be mentioned. Thorium first breaks down into mesothorium I., which in eight years has half broken down into mesothorium II., which again is half gone in nine hours, changing into radiothorium and throwing off β and γ rays; radiothorium then throws off an α particle (an electrically-charged helium atom) and becomes thorium X. Thorium X breaks down into thorium emanation, which is a gas having a half life period of only seventy-seven seconds, and breaks down with loss of an α particle into thorium A which is followed by other products B.C. and D., the final product being as yet unknown. Owing to the fact that considerable quantities of thoria are used in commerce for the manufacture of incandescent gas mantles, it has lately been possible to extract appreciable amounts of mesothorium and radiothorium.

The monazite sand is treated in some way devised by Dr. Hahn, so as to separate the mesothorium and so on from the thorium, and such preparations have lately been put on the market by the firm of Knöfler & Co., and will doubtless be used largely for medical purposes.

Professor Soddy has found that mesothorium and radium are identical in chemical properties—a very remarkable fact. He points out, too, that thorium, ionium, radiothorium form one group of radioactive elements, and mesothorium, radium, thorium X form a second group which are chemically identical and inseparable elements, although their atomic weights differ by two units in each case; while the last member of the first group and the first member of the last group (radiothorium and mesothorium) possess the same atomic weight and yet are chemically different.

THE NATIONAL PHYSICAL LABORATORY.—The annual meeting of the General Board at the Laboratory, Bushey House, Teddington, was held on Friday, March 17th, and in the afternoon a number of guests had the privilege of going over the Laboratory. They were received in the building containing the National Experimental Tank by Sir Archibald Geikie, who is the Chairman of the General Board. All the departments were thrown open and members of the staff were at hand to explain all the work which is carried out.

THE ROYAL AUTOMOBILE CLUB'S SUGGESTED LABORATORY.—We learn from the Secretary of the Royal Automobile Club that the Expert and Technical Committee has been asked to consider the expediency of establishing a central research laboratory for the scientific investigation of motor car problems and to report as to the equipment and maintenance to such a laboratory.

XUM

ZOÖLOGY.

By PROFESSOR J. ARTHUR THOMSON.

THE BIOLOGICAL HOROSCOPE.—Professor D. H. Tennent, of Bryn Mawr, has been very successful in crossing sea-urchins of different genera, *Toxopneustes* and *Hipponoë*. The Toxopneustes influence dominates in sea-water of a higher OH ion concentration, and the Hipponoë influence dominates in sea-water of a lower OH concentration. It is suggested that this variation in the sea-water, brought about artificially in the laboratory, may correspond to normal seasonal changes. If so, it throws light on the difference previously observed between the winter embryos and the summer embryos of the cross between *Sphaerechinus* and *Strongylocentrotus*. Thus something depends on the *season of birth*, and thus we come back to the horoscope and astrology—in new guise, of course.

SPARROWS AND POULTRY.—The poultry-raising industry in many parts of the United States is seriously menaced by "blackhead" and similar diseases due to parasitic Protozoa known as Coccidia. Philip H. Hadley has found that these parasites are abundant in the intestinal tract of the English Sparrow, which he therefore blames for the diffusion of the disease. The parasite occurs also in some other wild birds, such as the American "robin" (*Merula migratoria*), and severe coccidiosis has been observed in the quail (*Colinus virginianus*) and the grouse (*Bonaso umbellatus*). Thus wild game birds as well as poultry (fowls, turkeys, ducks, geese, pigeons, pheasants, guinea-hens) are seriously threatened.

LEPTOMONAD IN EUPHORBIA.—Not long ago Lafont made the remarkable discovery that the latex of *Euphorbia pilulifera* in Mauritius contained as a parasite a species of *Leptomonas*, that is to say, a kind of parasite characteristic of animals, and next door to Trypanosomes which cause sleeping sickness and the like. This very interesting discovery has been confirmed by G. Bouet and E. Rouband in regard to other species of Euphorbia. They find the infection local, temporary, and without obvious pathological effects. They were led to regard a small Hemipterous insect, *Dicuches humilis*, as the infecting agent, but fresh experiments by Lafont, in the case of *Euphorbia pilulifera*, point to another bug, *Nysius euphorbiae*, as the culprit.

CILIARY AND MUSCULAR MOVEMENTS.—Looking down from the rocks into the water in the summer season one often sees a jellyfish and a Ctenophore moving together, both very beautifully, but in very different ways; for the jellyfish is moving mainly by muscular contraction and the Ctenophore by cilia. It has been shown by A. G. Mayer, in a series of interesting papers, that these two kinds of movement have a converse relation to one another. Thus sodium is the most potent check to ciliary activity and the most powerful neuro-muscular stimulant. Magnesium is most potent in maintaining ciliary movement and the most powerful inhibitor of neuro-muscular movements. Among reägents of this sort whatever stimulates cilia depresses muscular activity, and whatever inhibits muscular movement stimulates cilia. In nature the more highly specialised cilia, such as those of the Ctenophore's combs, which are under the control of the neuro-muscular system, stop whenever the muscles contract and beat only when the muscles are relaxed. "The discovery of this converse relation makes very apparent the incompleteness of all existing explanations of the cause of animal movements."

REVIEWS.

CHEMISTRY.

New Reduction Methods in Volumetric Analyses.—By E. KNECHT, Ph.D., F.I.C. and EVA HIBBERT. 108+x. pages.

(Longmans. Price 3/- net.)

This little volume gives, in an accessible form, the numerous papers published by the authors in different journals upon the use of titanous chloride in volumetric analyses. Solutions of this salt can now be readily obtained in a fairly pure condition and its very powerful reducing properties render it particularly suitable for use in the volumetric estimation of all kinds of compound. Full descriptions of the methods of using it are given here, and in the case of some of these estimations there had previously been no reliable process of determining the substance in question. In particular, the application of this compound to the quantitative estimation of various dyestuffs will be found especially useful. The book will prove a valuable addition to the library of every laboratory.

BIOLOGY.

The Origin of Species by Means of Natural Selection (Popular impression of the copyright edition).—By CHARLES DARWIN. 432 pages. 7½-in.×5-in.

(John Murray. Price 1/- net.)

"The Origin of Species" has now passed out of copyright, but the edition which it is now open to anyone to print is the unrevised one which was superseded by the present one. Mr. Murray is to be congratulated on bringing out the popular impression at a price which is within the reach of everyone.

BOTANY.

Life Histories of Familiar Plants.—By JOHN J. WARD, F.E.S. 204 pages. 86 plates. 8-in.×5-in.

(Cassell & Company. Price 3/6.)

This popular edition will give Mr. Ward's delightful essays on the familiar plants a wider circle of readers and will help on the great movement that is proceeding, whereby the public generally is being interested in the world of life around it.

Open Air Studies in Botany.—Second edition. By ROBERT LLOYD PRAEGER, B.A. 266 pages. 68 illustrations. 8-in.×5¼-in.

(Charles Griffin & Company. Price 6/- net.)

We are pleased to see that a second edition of Mr. Praeger's book has been called for as it is not occupied with the dry-as-dust Botany, but with sketches of British wild flowers in their homes among the shingle, by the river, in the meadow, among the corn and along the fragrant hedgerow. Those who know Mr. R. Welch's photographs will know that the plates well illustrate the plant associations and habitats with which they deal.

Aids to Bacteriology.—Second edition. By C. G. MOOR, M.A., and WILLIAM PARTRIDGE. 240 pages. (6½-in.×4-in.)

(Baillière, Tindall & Cox. Price 3/6 net.)

This book is a second edition of a little work dealing briefly with a great number of points coming under the heading of Bacteriology. In the Introduction general matters are considered and the various bacteria of disease, methods of examining them, forms producing fermentation, and the bacteriology of every-day life are briefly touched upon.

XUM

ELECTROGRAPHS.

By A. W. CLAYDEN, M.A.,

Principal of the Royal Albert Memorial University College, Exeter.

SEVERAL years ago, when experimenting with an early form of Tesla apparatus supplied to the College by Mr. Apps, I hit upon a phenomenon which deserves to be better known. At that time I believed the results were novel, but when I came to describe them I found that similar observations had been made by Mr. F. J. Smith at Trinity

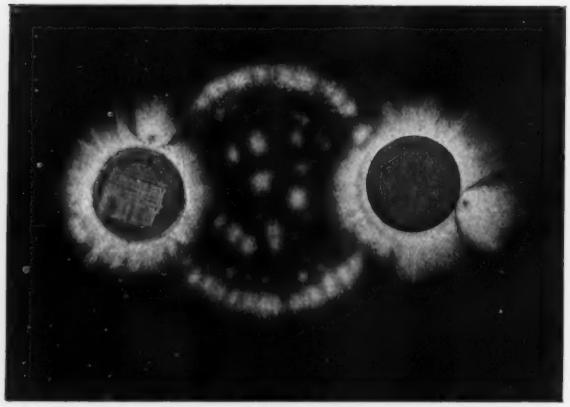

FIGURE 1.

Electrograph of a large coin (blurred image) resting upon two small medals.

College, Oxford, and described by him on June 24th, 1892, to the Physical Society under the title "Inductoscript." His method was to place a coin or medal upon a photographic plate which rested on a conductor, and then connect the coin and conductor with the poles of an "Inductorium" or Transformer for a time varying from five seconds to fifty seconds. On development the details of the face of the coin in contact with the film were revealed. His paper appears in Volume XI of the *Proceedings of the Physical Society*, but unfortunately it is not illustrated by any reproductions of the photographs.

Seven years later, struck by the beauty of the discharge from the Tesla transformer, it occurred to me that if a number of metal bodies were to be placed upon a sensitive plate it should be possible to get some sort of reproduction of the surrounding discharges.

If, for instance, a brass sheet is laid on the working bench, then a plate of glass, and a coin or group of coins upon the glass, on connecting one of the coins with one terminal of the transformer and the brass plate to the other terminal the coins are seen to be surrounded by beautiful radiating coronae caused by the discharge. I therefore substituted a photographic plate, film side uppermost, for the glass and let the discharge pass for a second or two. On developing, I was surprised to see that not only were the details of the radiating discharge far more perfectly rendered than I had expected, but the modelling of the whole face of the coin in contact with the film was sharply revealed.

In order to get the detail it was necessary to have the coin actually resting on the film. In some cases it was lifted up a short distance, and the mere thickness of a threepenny bit was found to be enough to spoil and blur the image. See Figure 1, in which the central large coin rests upon the little medals.

Mr. Smith attributed his photographs to the electric current, or at least the electrification of the film, but in the case of my observations it is not easy to say whether the action on the film was due to the discharge or to the light of the discharge. A coin under the conditions of the experiment, that is to say, in contact with one terminal of a Tesla transformer, is luminous all over, and the longest rays spring from the sharpest convexities, such as the the edges of the milled rim, in accordance with the general rule as to the distribution of a charge on conductor.

I am inclined to think that this distribution of the discharge is the real explanation of the phenomenon, though the effect on the silver salt may just as well be electric as strictly photographic.

FIGURE 2.

The effect of the sparks connecting coins and in the square hole of the Japanese example.

However this may be, the experiments are very easy to make, very beautiful to see, and yield results of considerable interest. Note, for instance, the sharp, slender sparks which connect the various coins, the corona of radiating sparks surrounding each coin, and the way in which these coronae decline to join. This last feature is especially well shown in the square hole in the Japanese coin shown in one picture (Figure 2), and in the discharge

surrounding the coin common to the arms of the cross in another (see Figure 3).

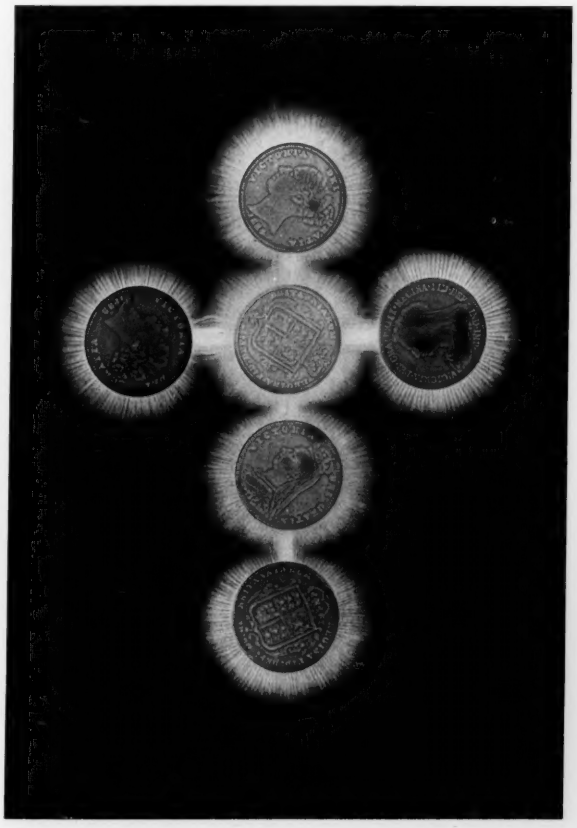

FIGURE 3.

Coins arranged in the form of a cross.

One specially interesting photograph (Figure 4) is perhaps that which shows a single coin and seven small dots. This was taken by connecting one terminal with the coin, then placing some small shot on the plate (the dots) at different distances, and finally connecting the other terminal with a ring of brass wire about six inches in diameter and concentric with the coin. The discharge was passed for one second and the plate then developed.

The corona round the coin at once calls to mind the solar corona, and the luminosities attached to the shot suggest comets' tails. Each shot has two fans of light, a broad one pointing away from the centre, which is larger as the shot is nearer, and a narrower sheaf connecting the shot with the coin, which dies out with increase of distance much more rapidly than what may be called the outer tail.

Without trying to found any argument as to the nature of comets' tails and the light sometimes seen pointing from a nucleus towards the sun, the pictures are certainly very suggestive, and the actual discharge as seen by the eye is much more strongly so.

However, space in these pages is valuable, and enough has been said. The phenomena are in the main only what the known facts of the brush discharge would suggest, except that I feel sure no one would have supposed that it would be possible to reproduce such detail without the use of any lens or camera.

Inductoscripts, electrographs, or mere contact photographs, whichever they may be, I think they are interesting enough and curious enough to be made more common knowledge.

The apparatus I used was an induction coil giving a six-inch spark, by which two medium-sized Leyden jars were charged. The discharge from these was then sent through the primary of a small oil-insulated transformer, and the length of the spark gap adjusted until the wires from the terminals of the transformer were as luminous as it seemed possible to make them.

The coil, jars, and spark gap were covered by a large cardboard box so that the photographic plate was not fogged by stray light, but was exposed only to the action to be examined.

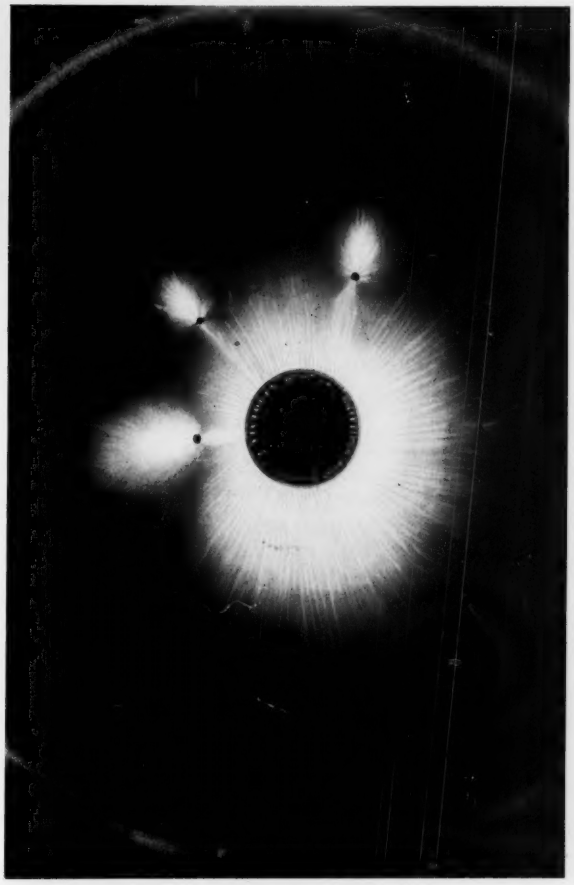

FIGURE 4.

Seven shot surrounding a coin.

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THE PROBLEM OF THE ROTATION OF VENUS AND THE INFERENCE TO BE DRAWN FROM THE PROBABLE ATMOSPHERIC CONDITION OF THE PLANET.

By B. G. HARRISON, F.R.A.S.

In connection with the problems of tidal friction and planetary evolution the question of the rotation of Venus is of the utmost importance. Unfortunately its period is still a matter of considerable uncertainty, but is generally considered to occupy either about twenty-four hours, or else to be equal to the planet's orbital revolution of two hundred and twenty-five days. Although most astronomers favour the latter alternative, there are serious objections to either, and it is quite possible there may be another solution to the question. There now seems little doubt that Mercury always turns the same face to the Sun, but the case is not nearly so certain with regard to Venus, either from a dynamical or observatorial point of view.

Indeed, all the early astronomers assigned to the planet a period of about twenty-four hours, but their data seem to have been somewhat insufficient, owing perhaps, to the time of day at which their observations were carried out. The most favourable time is between sunrise and sunset, as the planet is then high in the heavens and its glare is not so pronounced, on account of the brightness of the sky, while it is also possible to observe any peculiar features that are presented for several consecutive hours. Until recently, however, all observations were carried out either shortly before sunrise or just after sunset, and consequently at much the same time each day, so that if any markings were noticed on the planet in the same position on successive days, it may have been attributed to a revolution having been completed in the interval.

Until the observations of Schiaparelli, in 1889, the idea of a period of two hundred and twenty-five days does not seem to have been seriously considered, but since then many eminent astronomers, including Professor Lowell, have come to the same conclusion.

If it could be established that Venus were cloud-covered, it would render this latter alternative very improbable. For if the planet always turned the same face to the Sun, unless its thermal conductivity is infinitely greater than that of the Earth, the temperature on the dark side would be certainly low enough to cause precipitation of the moisture contained in the hot winds blowing from the sunward side. There would be a rapid and constant circulation of air owing to the difference between the temperatures of the two hemispheres, and evaporation and condensation would be continuous. As the supply of water could not be replenished from the dark surface owing to its conversion into ice, all water would long ago have ceased to exist in liquid form, which would entail a consequent absence of cloud from the planet's atmosphere.

It is, of course, possible that the surface of Venus still has a certain amount of intrinsic warmth, and in that case the dark side may be in a similar condition to the Devonian and Carboniferous periods here, as the clouds would, to a great extent, prevent the access or escape of heat. Now, if we accept this as an argument in favour of a day equal in length to two hundred and twenty-five of ours, we must also bear in mind the considerable difference in the relative ages of the planets which the supposition would necessarily involve. As the density of Venus is only ·85 and its mass ·75 of that of the Earth, the former would not only have generated less heat by contraction, but would also radiate it more rapidly and must, therefore, have been evolved much more recently to be still in a more primitive condition than the Earth. The minimum period of terrestrial rotation is unknown, but was possibly five hours. If, however, we assume that the Earth and Venus each had an original rotational velocity proportional to their masses, and that that of the former has been diminished to its present speed by lunar and solar friction, it is obvious that the more recent the formation of Venus is considered to be, the less likely is it that it should have already reached its maximum rotational period owing to secular tidal action.

There is no doubt that a dense envelope of some sort surrounds the planet. The appearance of a luminous ring and the "black drop" observed at recent transits, are alone conclusive proof of this, while the irregular and graduated appearance of the terminator also furnishes us with further evidence of its presence. Then, again, the extraordinarily high albedo of Venus almost prohibits the assumption that it is the actual surface we see. Moreover, the dark markings which have been frequently observed scarcely exhibit the permanency one would expect in the case of furrows on the face of the planet, and it is quite possible they may be caused by convection currents effecting openings in the clouds. In any case, it is difficult to understand how it would be possible to see any detail on the surface itself when one considers this is probably surrounded by a mirror which reflects 92 per cent. of the light received. It is this very high reflective index which presents some difficulty to the theory of a cloudy atmosphere. Until recently cloud was considered to be the highest reflective surface of a planet. Now as its albedo is only 72 it seems scarcely enough to

account for the brilliancy of Venus and this point has been brought forward as an objection to the cloud theory. Since it was necessary to give some alternative explanation, it has been suggested that the light is reflected from minute particles of dust suspended in the atmosphere, but this idea does not appear altogether satisfactory and it is possible to find another reason for the unusual brilliancy of the planet. The mean temperature of the air on Venus must be considerably higher than that of the terrestrial atmosphere, and should be taken into consideration. This would necessarily involve the attainment of a higher altitude and diminished pressure for the formation of clouds, which would, therefore, be probably thinner than ours, and would be covered by a film of cirrus extending almost to the limits of the atmosphere. It is well known that an attenuated substance is capable of reflecting more light than when condensed, and the rarefied condition of the clouds might thus account for the abnormal albedo of the planet. The peculiar appearance of the terminator, caused by the prolongation and occasional detachment of the cusps, can also be most easily explained by the theory of cloud reflection.

Another peculiarity has been noticed more than once when Venus has been examined under suitable conditions. The dark portion of the sphere has been found to emit a faint luminosity similar to that observed on the new moon. We know this latter spectacle to be caused by the reflection of light from the earth on the dark portion of the lunar surface. In the case of Venus, there is no one body capable of transmitting the necessary illumination, and its cause has always been rather a mystery to astronomers. Now this phenomenon, which for want of a better name may be termed "starshine," has been advanced as an argument in favour of a rotation of two hundred and twenty-five days. It has been pointed out earlier in this article that under these conditions if there were water on the planet it must all have been deposited on the dark side in the form of ice; and it is suggested by supporters of this rotational theory that the ghostly appearance sometimes observed is caused by the light from the Earth and stars *mirrored on the ice.* A moment's consideration will, however, show the impossibility of this idea. It is generally admitted that the whole sphere is surrounded by a dense atmosphere, and this would be quite sufficient to prevent the greater portion of starlight from reaching the surface, even in the absence of clouds. When one considers that the sunward side, where no ice can exist, is capable of transmitting over 90 per cent. of the light received, it is hardly necessary to invent an ice mirror to account for the "starshine" on the dark portion of the planet.

The evidence of the spectroscope shows that water vapour is probably present on Venus and thus this instrument adds its testimony to the likelihood of a cloud-laden atmosphere. At the same time the spectroscope gives no evidence of rapid axial rotation. It is well known that a rotating body, which emits or reflects light, displaces the lines of the spectrum. This gives the latter a twisted appearance, owing to one limb of the sphere approaching the observer, whilst the other recedes, and thus altering the relative wave-lengths of the light. The amount of this alteration for a given speed of rotation is well known, and as it is possible to obtain by these means the angular velocities of the Sun, or any body whose diameter is known, and compare them with the results obtained by visual observation, the accuracy of this method has been indisputably proved. Naturally, the lower the velocity happens to be, the greater is the difficulty experienced in its measurement by these means, but in the case of a body having a diameter of seven thousand seven hundred miles, any motion involving a rotational period of less than two months might be detected.

As the spectroscope has failed to show any shift in the spectral lines, this presents an almost insuperable obstacle to a period anything like so short as twenty-four hours. We have also seen that a rotation, which would cause the planet to turn the same face always to the Sun, is incompatible with the presence of water. We are thus led to conclude that the planet's day is between two months and two hundred and twenty-five of our days in length.

This is rendered more probable since the slow axial rotation which this hypothesis necessarily involves would be almost impossible to detect from observation. Schiaparelli himself admitted that the planet's day might be any period between six and nine months in length, and it was, therefore, concluded somewhat prematurely that if Venus had no visible rotation it had already reached the stage when the same face was always turned to the Sun. If this has happened there must have been an intermediate stage when the planet's day was shorter than its year, but still not short enough to enable us to detect any motion, *and there is no evidence to show that this stage has yet been passed.* With the exception of Mercury all the other principal planets appear to have rotations of twenty-four hours or under, and if their periods are eventually lengthened until they are synchronous with their respective orbital revolutions, the time required for this process will be extended as their velocities decrease, owing to the consequent reduction of tidal friction. This friction will also be further diminished since time will cause an increasing rigidity of each planet on account of its gradual loss of heat, and in the case of the inner planets through the escape of water vapour by molecular activity. Consequently, the final stage of rotational independence will be prolonged for a considerable period before it is completely controlled by the sun, and this renders it likely that Venus still has a somewhat shorter day than its year. Moreover, the only friction capable of acting on Venus is that generated by the solar tides, and although, as their capacity for retardation varies inversely as the sixth power of the distance,

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these are seven-and-a-half times more effective than those on the Earth, the rotation of the former planet has never been checked by lunar action, which in our own case may have been an exceedingly powerful factor in the earlier stages of evolution. It therefore does not seem improbable even from a physical standpoint that the length of the Cytherean day is shorter than its year, and this supposition would meet most of the difficulties we should otherwise have to contend with. There is also, of course, the possibility of the planet having a slow retrograde motion, although this is very unlikely on purely dynamical grounds.

Altogether the importance of this rotational question can scarcely be over-estimated, and the necessity of obtaining further information on a subject about which so much uncertainty exists, opens up an admirable field of research for amateurs.

REVIEWS.

PHYSICO-CHEMISTRY.

Théorie Physico-Chimique de la Vie et Générations Spontanées.—By STÉPHANE LEDUC (Professeur à l'école de Médecine de Nantes), Paris. 204 pages. 57 illustrations.

(A. Poinat. Price 5 francs.)

This little book gives an excellent summary of some of the most important physico-chemical processes that take place in living organisms, and in particular of osmosis. The author shows by a series of extremely interesting photographs (of which one is reproduced in Figure 1), that it is possible by introducing mixtures of certain salts into solutions of other salts to produce, by osmotic pressure, forms analogous to those of plants and the shells of aquatic animals. These forms may be used as illustrations of the mechanism by which the forms of living organisms were possibly produced, but, in our judgement, it is straining analogy of form too far to regard them as evidence of spontaneous generation. The author does not succeed in bridging the gulf between the force that causes a crystal to form in a certain way and the force that we know as "life." He refers also to the analogy between Bredig's so-called "inorganic ferments" and enzymes such as pepsin and diastase. In both cases the action is inhibited by the presence of certain poisons, but whereas colloidal platinum may be rendered active over and over again, an enzyme such as diastase is destroyed by a moderate degree of heat, and its activity can never be restored. In fact, it might almost be possible to form an enzymic definition of life. When once an enzyme has been produced without the aid of a living organism the possibility of proving spontaneous generation will be nearer than it is at present.

By the courtesy of Mr. Deane Butcher *Editor of "The Archives of the Röntgen Ray."*

FIGURE 1. Osmotic Growths.

PHYSICS.

Radium: Its Physics and Therapeutics.—By DAWSON TURNER, M.D., F.R.C.P. 86 + x. pages.

(Baillière, London, 1911. Price 5/- net.)

In this book, Dr. Turner has summarised in a concise yet readable form the various researches published in scattered scientific journals upon the physical phenomena of radium. He has also given a description of the uses to which radium has been put as a curative agent, illustrated by outlines of the cases and photographs, which are obviously more intended for the medical than for the general reader. It is in fact with the therapeutic part of the subject that the book mainly deals, and it appears to us to be admirably suited as a guide to medical men who desire a handy manual upon radium.